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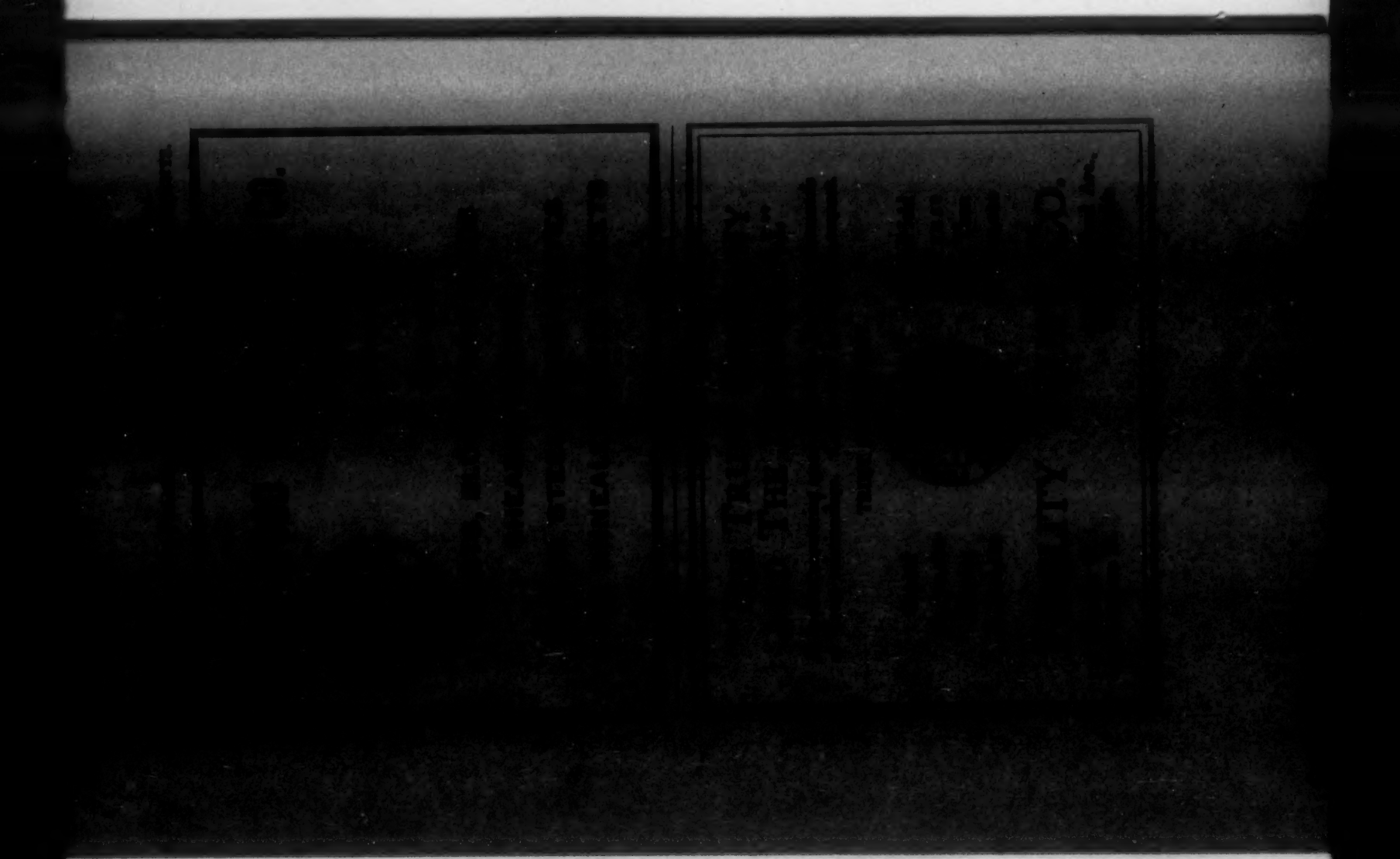

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THE STRUCTURE OF THE ATOM.*

BY

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THIRTY years ago, the science of physics was in a very lifeless state. There was a prevailing belief that all that was worth doing had been done; and, if anyone wished to devote himself to research in the subject, he would usually turn his attention to remeasuring some of the fundamental physical constants, such as the mechanical equivalent of heat, or the constant of gravitation, in the hope that, by patient effort, he might add another decimal to the results obtained by his predecessors. Even at that time there existed in the show-cases of our physical laboratories curiously shaped glass tubes embodying all the art and skill of the glass blowers, which could be made to glow in remarkable and fantastic ways; and, on the occasion of any scientific festivity, they would be hauled out, made to go through their performances for the mystification of anyone who cared to look at them, after which they were returned to their cases to rest until the next function of the kind. Everyone realized that something remarkable was going on in those tubes; but few realized, at the time, that they contained the embryo of most of those discoveries which were

* Presented at the Stated Meeting of the Institute held Wednesday, December 19, 1923.

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destined, in the next quarter of a century, to raise the atomic theory of matter from a vague speculation with hardly the shadow of a hope of experimental realization to a position of surety which, at the present time, no one would dream of questioning.

Thirty years ago we knew that there were such things as atoms and molecules; and, indeed, we knew the relative weights of the atoms, the atomic weights, in terms of hydrogen, the lightest, as unity; but, our knowledge of the individual atoms and molecules was very vague, and we felt that we had penetrated quite far into the mysteries of nature in being able to say that, on the basis of certain plausible considerations, it was probable that, roughly speaking, the molecule was of such size that, if a drop of water should be magnified to the size of the earth, the molecules would, on the same scale of magnification, become about as large as oranges. Nobody ventured to picture for the molecule any structure more elaborate than that of a hard sphere. Even this simple conception served to clarify many facts concerning the behavior of gases. Thus, on the assumption that these spheres were in rapid motion, it became clear how a gas could exert a steady pressure on the walls of a vessel containing it, just as it was clear how a sand blast directed on the hand could produce the equivalent of a steady pressure. It was clear how it should be possible to reduce the volume of a gas so greatly, since the distances between the molecules might be many hundreds of times the molecular diameter; and, it was clear why such reduction in volume is accompanied by increase of the pressure which the gas exerts; for, reduction in volume would obviously increase the frequency with which each individual molecule struck the wall of the container. Moreover, if heating a gas was to be considered as synonymous with increasing the agitation of its molecules, many of the phenomena associated with heat were explained. Thus, the equalization in temperature between a hot and a cold mass of gas would simply mean the passage of kinetic energy from one part to another, with the consequent reduction of the molecular agitation of the hotter part. As to the sizes of these molecules, and the number of them in a cubic centimetre, however, we were in almost complete darkness, and our knowledge of their internal structure was still less.

To-day all of this is changed. We know the number of molecules in a cubic centimetre of a substance with a greater accuracy

than we know the population of New York City. We know the weights of these molecules with an accuracy probably greater than that with which anyone in this room knows his own weight at the present time; and, while we have yet much to learn as to the structures of the atoms and molecules, we are well on the highroad to a solution of that mystery also.

In speaking of the atoms, one must accustom himself to think of numbers which are very large, and of numbers which are very small. An ordinary glass of water contains about ten million million million million molecules. In order to give you an impression of how large a number this is, I should like to quote an illustration which was first given by Doctor Aston, and which will bear repetition. Suppose we should take the glass of water, empty it into the Pacific Ocean, and, after waiting until it has become thoroughly mixed up with all the waters of the world, suppose that we should go to the nearest faucet and fill the glass once more. How many of the original molecules will be found in the glass? The answer is, about two thousand. The mass of the hydrogen atom is about one and six-tenths' times the one-millionth of the one-millionth of the one-millionth of the one-millionth of a gram. This means that, if we could increase the mass of everything until the mass of the hydrogen atom became equal in mass to 100 grams, that 100 grams would, on the same scale of magnification, become as heavy as the earth.

There is, however, something far lighter and smaller than the hydrogen atom, and that is the electron, whose discovery by Sir J. J. Thomson in 1898 was the beginning of a new era in physics. The electron is a particle of negative electricity having a mass only one-eighteen-hundredth of the mass of the hydrogen atom, and a size so small that, even in comparison with the size of the atom, it is but as a fly compared with a cathedral. By suitable means, electrons may be obtained from all substances; but, it is found that from whatever source, or in whatever way they are obtained, they all carry the same charge and have the same mass. Indeed, the electron appears to be a common constituent of all substances, and it is natural to regard it as one of the fundamental bricks out of which the atoms are built. Of course, a neutral atom must contain as much positive as negative electricity; but, at any rate until quite recently, we knew much less about the positive electricity in the atom than about the negative.

Any theory of atomic structure must naturally be built up in the light of the facts which it is the object of the theory to correlate. Apart from chemical phenomena, which it is not my purpose to discuss in any detail in this paper, there are two classes of phenomena in which the internal mechanism of the atom plays a vital part in what we actually see. The first of these concerns optical, and the second radioactive phenomena.

I would remind you that if a beam of light from a glowing solid be passed through a triangular block of glass, it becomes spread out into a band of color, a spectrum as we call it, red at one end and violet at the other, with all the colors of the rainbow between. We believe, or at any rate before the days of the quantum theory we used to believe—for now we do not know whether we believe it or not—that light is a wave motion in an ether which pervades all space, and that each of the individual colors corresponds to a definite wave-length. The longest waves which affect our eyes are the red ones, whose length is about seven one-hundred-thousandth of a centimetre, and the shortest are the violet, with a wave-length of about half this amount. This limited band of wave-lengths to which our sight is sensitive constitutes, however, but a small fraction of the waves with which modern physics deals. Proceeding to that region which lies just beyond the red, we encounter waves which, while they do not affect our eyes, will warm a body upon which they fall. Proceeding to yet longer waves, waves whose length is comparable with a centimetre, we begin to encounter the outskirts of that region which is concerned with the waves of electrical oscillations; and, there is a continuous gradation between these and waves comparable in length with a mile, which constitute the waves of wireless telegraphy. If we now start to explore the spectrum in the region beyond the violet, we encounter the so-called ultra-violet rays which, while we cannot see objects by them, possess the property of strongly affecting a photographic plate, and of producing marked therapeutic action. As we proceed still farther beyond the violet, we eventually meet the X-rays, which are of a length of the order one thousand-millionth of a centimetre, and finally, as we go still farther, we meet types of X-ray which man has not yet been able to produce, but which are produced spontaneously by the disintegration of the atoms of radioactive substances, and are known as gamma rays.

The spectrum of an incandescent gas presents quite a different state of affairs. It no longer shows the continuous band of color which we obtain from a glowing solid, but only certain isolated lines corresponding to perfectly definite wave-lengths. These lines are characteristic of the gas which emits them. We believe them to be determined by vibrations going on inside the atom itself, and therefore to hold, in part, the clue to the unravelment of the structure of the atom.

The second avenue of attack on the constitution of the atom is through the examination of those remarkable phenomena shown by the radioactive substances like radium, which are in a continual state of spontaneous disintegration. The time taken for any given mass of radium to diminish to half of its original amount is about 2000 years. As the radium dies, it gives rise to another substance, a gas, known as radium emanation. This gas is much shorter lived than radium itself. Half of any given quantity of it disappears in about four days; and, as it dies, it gives birth to another substance, radium A, whose half-life is only about three minutes. This in turn dies, giving rise to radium B, whose half-life is about twenty minutes and so on. Now when one of these atoms dies and gives rise to the next in succession, one or all of three types of radiation may be emitted. First we have the alpha particle, a positively charged atom of helium moving with a velocity of about 18,000 miles per second. Then we have the beta particle, which is an ordinary electron traveling with a speed comparable with that of light, a speed of 186,000 miles per second; and, finally, we have the gamma ray, to which I have already referred, and which differs from what we know as the X-ray only in having a shorter length and in being more penetrating.

One of the first suggestions made for a model of the atom, after the discovery of the electron, was that made by Sir J. J. Thomson, to the effect that the positive electricity in the atom exists in the form of a sphere whose dimensions are comparable with those of the atom itself, the electrons being embedded in the sphere like plums in a jelly. It was supposed that when one of these electrons was displaced from its position of equilibrium it executed vibrations about that position until it finally came to rest. These vibrations were supposed to be communicated to the ether and to constitute the light emitted by the atom; and, the

advantage of the model lay in the fact that the vibrations emitted under these conditions would not alter in their frequency as the amplitude of the motion of the electron died down. Such a requirement is very essential if we are to account for the fact that the lines of the spectrum of a glowing gas are as sharp as observation shows them to be; and, it is a requirement which is not very readily satisfied in other types of model.

With great ingenuity Thomson proceeded to show how such a model could be made to reflect the chemical properties of the atoms as we know them. I must remind you that the chemist has arranged the elements in a table, Fig. 1, in order of their atomic weights beginning with hydrogen, the lightest. One of the characteristic features of this arrangement, which it must be the aim of any theory of atomic structure to account for, is the fact that, as we proceed along the table in the direction of increasing atomic weight, we find a periodic recurrence of properties in the elements. Thus, taking lithium (Li), for example, we have an element whose chemical properties are very similar to those of sodium (Na), and of potassium (K) and of rubidium (Rb) and of cæsium (Cs). We have a similarity between beryllium (Be), magnesium (Mg), calcium (Ca), strontium (Sr), barium (Ba) and radium (Ra). Again we have a resemblance between fluorine (F), chlorine (Cl), bromine (Br) and iodine (I). It is as though, in the process of building up the heavier atoms, certain structures which are responsible for the chemical properties reappear again and again. Now in the Thomson atom, the simplest neutral structure would be a positive sphere with a single electron inside. This electron will find a position of equilibrium at the centre. If we should have a positive sphere with two electrons in it, these would find positions of equilibrium along a diameter at a distance apart equal to the radius of the sphere. Three electrons would arrange themselves at the corners of a triangle, four at the corners not of a cube, but of a tetrahedron. With increasing numbers of electrons the complexities of the problem increase rapidly; but, in order to obtain information as to the sorts of arrangements of electrons which might be expected, Thomson studied the simplified case in two dimensions, *i.e.*, the case where the electrons are constrained to lie in a plane. It follows mathematically that it is always possible to persuade the

FIG. 1.

Periodic Classification of the Elements.

	I		II		III		IV		V		VI		VII		VIII & O			
1	1 H 1.008																	2 He 4.00
2	3 Li 6.94		4 Be 9.1		5 B 10.9		6 C 12.005			7 N 14.008		8 O 16.00		9 F 19.0				10 Ne 20.2
3	11 Na 23.00		12 Mg 24.32		13 Al 27.0		14 Si 28.1			15 P 31.04		16 S 32.06		17 Cl 35.46				18 A 39.9
4	19 K 39.10		20 Ca 40.07		21 Sc 45.1		22 Ti 48.1		23 V 51.0		24 Cr 52.0		25 Mn 54.93		26 Fe 55.84	27 Co 58.97	28 Ni 58.68	
		29 Cu 63.57		30 Zn 65.37		31 Ga 70.1		32 Ge 72.5		33 As 74.96		34 Se 79.2		35 Br 79.92				36 Kr 82.92
5	37 Rb 85.45		38 Sr 87.63		39 Yt 89.33		40 Zr 90.6		41 Cb 93.1		42 Mo 96.0		43*		44 Ru 101.7	45 Rh 102.9	46 Pd 106.7	
		47 Ag 107.88		48 Cd 112.40		49 In 114.8		50 Sn 118.7		51 Sb 120.2		52 Te 127.5		53 I 126.92				54 Xe 130.2
6	55 Cs 132.81		56 Ba 137.37		Rare Earths		72 Hf 180?		73 Ta 181.5		74 W 184.0		75*		76 Os 190.9	77 Ir 193.1	78 Pt 195.2	
		79 Au 197.2		80 Hg 200.6		81 Tl 204.0		82 Pb 207.20		83 Bi 209.0		84 Po (210.0)		85*				86 Nt (222.4)
7	87*		88 Ra 226.0		89 Ac (226)		90 Th 232.15		91 PAc (230)		92 U 238.2							

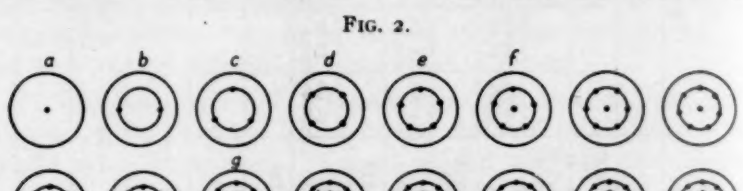
Rare Earths.							
57 La 139.0	58 Ce 140.25	59 Pr 140.9	60 Nd 144.3	61*	62 Sa 150.4	63 Eu 152.0	64 Gd 157.3
65 Tb 159.2	66 Dy 162.5	67 Ho 163.5	68 Er 167.7	69 Tm 168.5	70 Yb 173.5	71 Lu 175.0	

Atomic number at left of each element—atomic weight below.

electrons to remain in a plane provided that they are given a sufficient angular velocity of revolution about the centre.

The simplest case is then that of a single electron, which will arrange itself as shown in Fig. 2, *a*. Two electrons will arrange

FIG. 2.



	A	C														
Outer ring....	11	11	11	12	12	12	12	13	13	13	13	14	14	15	15	
2nd ring.....	5	6	7	7	8	8	8	8	9	10	10	10	10	10	11	
1st ring.....	1	1	1	1	1	2	3	3	3	3	4	4	5	5	5	
	B	D														
Outer ring....	15	15	15	16	16	16	16	16	16	16	17	17	17	17	17	17	17
3rd ring.....	11	11	11	11	12	12	12	13	13	13	13	13	13	14	14	15	15
2nd ring.....	5	6	7	7	7	8	8	8	8	9	9	10	10	10	10	10	11
1st ring.....	1	1	1	1	1	1	2	2	3	3	3	3	4	4	5	5	5
Outer ring....	17	..	..	19	20	20	20	20	20	20	20	20	20	21	21		
4th ring.....	15	..	..	16	16	16	16	17	17	17	17	17	17	17	17		
3rd ring.....	11	..	..	13	13	13	13	13	13	13	14	14	15	15	15		
2nd ring.....	5	..	..	8	8	8	9	9	10	10	10	10	10	10	11		
1st ring.....	1	..	..	2	2	3	3	3	3	4	4	5	5	5	5		
Outer ring....	21	..	..	22	..	23	..	24									
5th ring.....	17	..	..	20	..	20	..	21									
4th ring.....	15	..	..	16	..	17	..	17									
3rd ring.....	11	..	..	13	..	13	..	15									
2nd ring.....	5	..	..	8	..	10	..	11									
1st ring.....	1	..	..	2	..	3	..	5									

themselves as in Fig. 2, *b*, three, four and five as in Fig. 2, *c*, *d* and *e*, respectively. If, however, we try to persuade six electrons to revolve in a single ring, we shall find that they will refuse to obey. One will go to the centre, and the other five will form a ring around it as in Fig. 2, *f*. Having now paid our tax to the centre, we shall find that we can add additional electrons to the surrounding ring up to a total of eight. An attempt to add one more electron to the ring will, however, result in one electron going inside; and, if under this altered condition we try again to add an electron to the ring, one more electron will go inside, so that, at this stage, we really have two hings, Fig. 2, *g*, an inner ring containing three electrons, and an outer one containing eight. We may now go on adding up to ten electrons to the outer ring, after which, the addition of one more electron results in an inner ring with four electrons and an outer one with ten. The next electron goes to the inner ring, and the next to the outer

ring. A further addition of one electron to the system results in a state of affairs in which the electrons can no longer exist in two rings at all. One electron goes to the centre, forming the embryo of a third ring, while the remaining electrons arrange themselves in two rings outside. Fig. 2 indicates how the process goes on as we add more and more electrons, and shows that every now and again, a new ring makes its appearance in embryo, and with subsequent addition of electrons proceeds to grow up like its predecessors. Thus, for example, the three rings of the atom Fig. 2, A, are like the inner three rings of B, while the three rings of C are like the inner three of D, and so on. We might naturally expect some similarity between the properties of A and B and between the properties of C and D. The periodic recurrence of the properties of the elements is thus symbolized by the recurrence of rings which are alike. I must not pursue further the ingenious methods by which Thomson endeavors to find in this model a reflection of the properties of the elements in their relation to the periodic table of atomic weights. The problem of atomic structure has always presented two aspects, the physical and the chemical. Those theories which have been built up with a close regard to the physical facts have not been very fruitful in predicting the chemical, and those which have been built up on the basis of the chemical facts have not been very fruitful in predicting the physical. Since the main purpose of this paper is to lead up to a consideration of those remarkable developments in our ideas of the dynamics of the atom which have come from a study of its physical properties, I must pass over those interesting theories which have been developed by Professor Lewis and Doctor Langmuir to account primarily for the chemical behavior of the atom, and the still more recent work which Sir J. J. Thomson described to you a year ago, and which is published in the JOURNAL OF THE INSTITUTE.

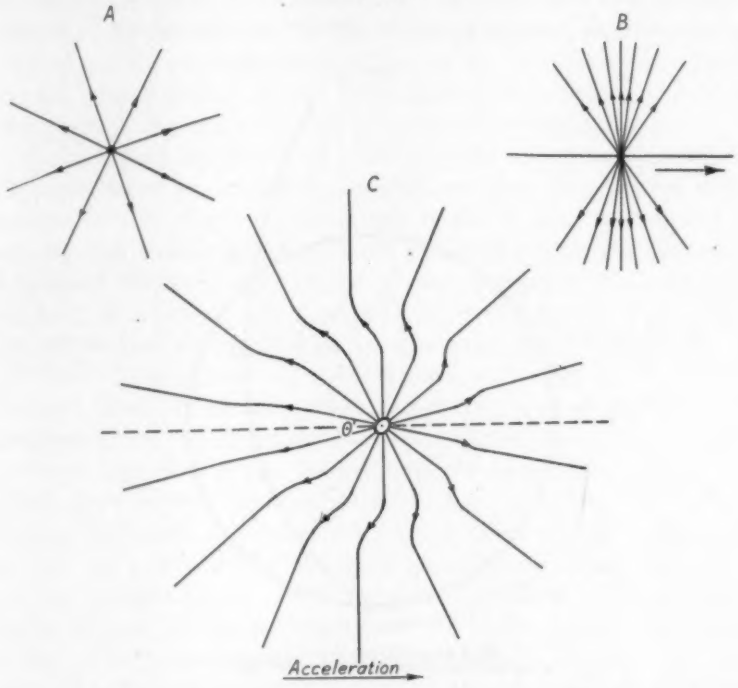
The characteristic feature of the old Thomson model is that, in it, the positive electricity exists in the form of a sphere whose dimensions are comparable with the size of the atom. Now we have certain means at our disposal for getting information as to how the positive electricity is distributed. The first of these is to be found in a study of the deflections suffered by alpha particles in passing through matter. If a beam of alpha particles is passed through a thin sheet of matter, the alpha particles which

emerge will be found to have all sorts of different directions. We must regard this deviation in path as ultimately traceable to the deflections which the alpha particle has suffered in passing near to the charges in the atoms, deflections of the same nature as those suffered by a comet in approaching the sun. Now, without entering into details, it will be reasonably obvious that the nature of the deflections, the relative numbers of alpha particles deflected through different angles, will depend upon whether or not the positive charge is concentrated in a very small region or is spread over dimensions comparable with that of the atom; for, in the latter case the alpha particle can never get as near to all of the positive electricity in an atom at the same time as it can in the former, so that sharp deviations are not as well provided for in the latter case as in the former. Thus, as a result of experiments on the deviations suffered by alpha particles in passing through matter, it turns out that we must regard the positive charge in the atom as concentrated in a very small nucleus. Other evidence leading to the same conclusion comes from a consideration of the origin of the mass of the atom. We have reason to believe that an atom contains comparatively few electrons, that the atom of hydrogen contains only one electron, that of carbon twelve electrons, and so on. Our evidence for this lies in the fact that if they contained many electrons, the effects resulting from the vibrations set up in them by the passage of X-rays through them would be greater than they are known to be. The total mass which it is permissible to assign to the electrons in the atom is far too small to account for the weight of the atom, so that it is to the positive electricity that we must look for the atom's mass. And here I should like to pause to discuss briefly how the mass of a charge is supposed to arise.

In the first place, I will remind you that an electric charge is surrounded by what we call an electric field, and we draw, from the charge, lines, called lines of force, or tubes of force, Fig. 3, *A*, whose direction everywhere represents the direction of the field, and whose number per square centimetre represents the strength of the field. If a charge is at rest, these tubes of force stretch radially outwards. The laws of electrodynamics tell us that if the charge is in motion with uniform velocity in a straight line, the lines of force also stretch radially outwards, although for high velocities they crowd towards the equatorial plane perpendicular to

the line of motion, Fig. 3, *B*. Suppose that we should suddenly increase the velocity of a charge from one value to another, and consider the state of affairs at some subsequent instant. There will be some places sufficiently remote from the charge where the field is as yet unaware of the alteration which we have made in

FIG. 3.

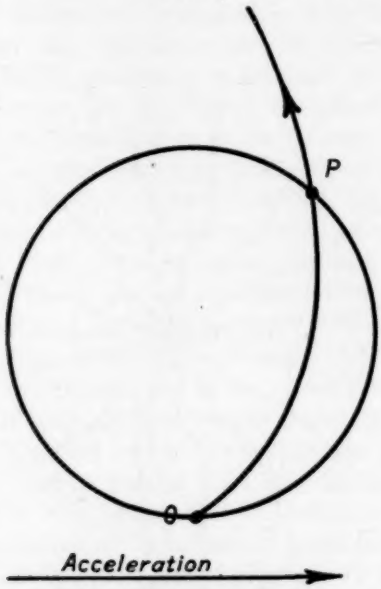


the charge's velocity, so that the tubes of force will here be pointed towards the place *O'*, Fig. 3, *C*, where the charge would have been had it kept its old velocity. On the other hand, there will be some points so near to the electron that the field has completely adjusted itself to the new velocity, and here the tubes will point towards the actual position of the charge. It is therefore obvious that the tubes will have to bend in the space intervening between the two regions. These tubes represent the direction of the field, and the fact that they are no longer radial shows that the field is no longer completely radial to the charge.

Suppose we now consider a positive sphere of electricity which

is suffering an acceleration in the direction of the arrow, Fig. 4. The tubes of force from some little element O will be bent in the manner I have described, so that the element O will exert on an element P a force which has a component opposed to the acceleration. In a similar way the element P will exert on O a force which has a component in a direction opposed to the acceleration; and, indeed, every element of the charge on the sphere will exert on every other element a force which is, in part,

FIG. 4.



opposed to the acceleration. The electron will thus create in itself a reaction to increase of its velocity, and this reaction being, as the laws of electrodynamics show it to be, approximately proportional to the acceleration, represents what experiment would reveal as the ordinary inertial reaction of the electron. If we divide this reaction by the acceleration we obtain the quantity which functions as the electronic mass. You will readily agree that, for a given total charge, the nearer the individual elements of charge are together the greater this reaction to acceleration, so that we might expect that the electromagnetic mass of the electron would increase with diminution of the electronic radius, for a

given total charge. As a matter of fact, a complete calculation of the effect shows that the effective mass is inversely proportional to the radius of the electron. We thus arrive at the rather astonishing conclusion that, since the mass of the hydrogen atom is about 1800 times that of the electron, and since the mass of the atom is to be regarded as concentrated wholly in the nucleus, the radius of the nucleus is only about 1/1800 of the radius of the electron, *i.e.*, the nucleus of the hydrogen atom is a sphere so small that, if you should magnify it to the size of a pin's head, that pin's head would, on the same scale of magnification, become magnified to the radius of the earth's orbit around the sun.

Once it had become recognized that the positive electricity of the atom must be regarded concentrated into dimensions small compared with those of the atom itself, it became natural to suppose that the electrons revolved around the positive charge in very much the same way as the planets revolve around the sun; for, only in this way was it possible to see how they could maintain themselves in equilibrium in opposition to the attraction of the nucleus. However, it soon became necessary to divide the electrons into two classes, those which revolved in this way, at distances from the central charge comparable with what we had formerly regarded as the diameter of the atom, and another set which, even if they revolved at all, did so in orbits which were much more closely associated with the positive charge. Thus, the modern view which the physicist takes of the atom is that it consists of these parts. First, we have a nucleus, which contains all the positive charge, and practically all of the mass of the atom. It may, and in most atoms must, also contain electrons as well, either in some sort of statical equilibrium, or revolving in orbits which are very closely associated with the central charge. Around the nucleus we have electrons revolving in orbits of a size comparable with the one-hundredth-millionth of a centimetre. These electrons are much less firmly bound to the atom than are the nuclear electrons. It is these electrons which, in modern theory, figure in the production of X-rays and light. It is, moreover, to them that we look for an explanation of those binding forces between atoms and molecules which we know under the names of chemical action and cohesion.

One of the greatest steps in the elucidation of the structure of the atom was made when, in 1912, Moseley examined the X-ray

spectra of the elements. Time will not permit me to enter into the details of Moseley's experiments further than to say that, by stimulating the electrons in the atoms of different substances by means of X-rays, he caused those electrons themselves to emit X-rays whose properties he examined, and he found that his experiments received a simple interpretation on the assumption that the net charges on the nuclei were proportional to the numbers which would be obtained by arranging all the atoms in a row in the order of their atomic weights, and then numbering them according to the position which they occupied in this row, starting with hydrogen, the lightest. In other words, if we should call the charge on the nucleus of hydrogen e, that on the next heaviest element, helium, would be $2e$, that on the next heaviest, $3e$, and so on. The number which designates the charge on the nucleus in terms of the charge on the nucleus of hydrogen is called the atomic number, and the nucleus of hydrogen itself is called the proton.

If we add one proton to the nucleus, we increase the atomic number by unity. If, however, we add a proton and an electron together, we do not change the charge on the nucleus at all. Its atomic number is the same as before, and the electrons describing orbits around it are quite unaware of the change which has taken place. The weight of the atom will, however, be changed. Insofar as we believe that the chemical properties of the atoms, their spectra, their behavior as regards X-rays, etc., and indeed, almost everything we can observe about them are determined by the orbital electrons, and since, as regards these, the nucleus figures only in respect of its net charge, we see that the addition of an equal number of protons and electrons to the nucleus would result in an atom which differed from that from which it was formed in no respect which we could observe, other than in the matter of its weight. Atoms which differ only in this way are called isotopes of each other. It is practically impossible to separate them chemically, because they are absolutely alike as regards their chemical reactions. Only by the utilization of some process which makes use of the differences in the atomic masses is it possible to separate them.

It turns out that a substance like mercury is composed of at least three isotopes, so that the atomic weight which the chemist has given us is a sort of average, depending upon the relative

amounts of each of these three isotopes present. Now when we make due allowance for all this, a very remarkable result follows. It had been noted by Prout, more than a hundred years ago, that the weights of the atoms were very nearly whole number multiples of the atomic weight of hydrogen, suggesting that hydrogen might be a fundamental unit out of which the other atoms were built up. Thus, if we call hydrogen unity, oxygen is very nearly sixteen, nitrogen is very nearly fourteen, and so on. There were, however, many notable exceptions like chlorine, which has a weight 35.5 times that of hydrogen, and the accuracy of present-day chemical measurements is, of course, such that it is out of the question to suppose that this could be a mistake for either 35 or 34. Now it turns out that when we speak of the atomic weights of the individual isotopes which we have been able to separate, and not of the average atomic weights of mixtures of them, we obtain values which are very accurately multiples of one single unit, that unit being approximately the atomic weight of hydrogen. We have seen that the electron is a fundamental brick out of which the atoms are made, and it now appears that the proton, the positive nucleus of hydrogen, is another. In fact, it seems that we now have strong evidence for the belief that the nuclei of all of the atoms may be regarded as made up of the constituents of atoms of hydrogen.

It is, of course, the nucleus which differentiates one atom from another; to change it would be to change the atom; and, until comparatively recently, the nucleus had resisted all attempts of man to disintegrate it. Of course, when a radium atom shoots out an alpha particle and becomes an atom of radium emanation, we have a case of such a change, a real formation of one kind of atom from another. Even in the case of the radium atom this is a comparatively rare event. For although a gram of radium shoots out thirty-four thousand million alpha particles in a second, that gram contains about three thousand million million million atoms of radium, so that the average life of an atom is about 3000 years. We should regard any concern whose constitution remained fixed for 3000 years as a pretty stable sort of a concern. Most of the atoms are very much more stable than radium, however, so stable that we cannot tell whether they would ever break up, in spite of the fact that the sensitivity of our measurements is such that we can tell that an atom of uranium,

for example, does occasionally break up, although the average life of the uranium atom is of the order ten thousand million years.

Such disintegrations of the atom nuclei as we meet with in the case of radioactivity are of a kind out of the control of man. There is nothing which we can do to hasten or retard them; and, until comparatively recently, the nucleus of the atom has resisted all attempts of man to change it in any way. Within the last few years, however, this last fortress guarding the secrets of the atom has been subjected to successful attack. In the very literal sense of the word, it has been bombarded. The guns have been atoms of radium C, and the cannon balls have been alpha particles shot out of them. An alpha particle is a very concentrated source of energy. Mass for mass, its energy is 400 million times that of a rifle bullet. If we fire an alpha particle into a piece of matter there is a chance that it will suffer a sufficiently good head-on collision with the nucleus of an atom to disrupt it. The chance of its doing this is not as great as you might think at first sight; for, as we shall presently see, the space within the atom is about as empty as our solar system is. The distances of the protons and electrons from each other are very large compared with their dimensions, so that while there would be but little room for the penetration of large things, there is plenty of room for small things. In fact, a good head-on collision of an alpha particle with a nucleus is a very rare occurrence. In most cases the nucleus of the alpha particle does not pass near enough to the nucleus of another atom which it approaches to do more than shake it, as a comet passing through our solar system might shake the sun. However, Professor Rutherford has found that if he shoots alpha particles through thin sheets of matter, a certain proportion of them do make sufficiently violent collision with the nuclei to disrupt them, and hurl out from them atoms of hydrogen. Of course, even in this case, the energy of the alpha particle has come from the radioactive atom which fired it. Its energy has not been imparted to it at the will of the investigator. But, an alpha particle is nothing more than a positively charged atom of helium moving with great speed, and it is not improbable that we shall in the near future succeed in artificially giving alpha particles the speeds they have from radium atoms, and of obtaining them in much greater numbers than we can hope to obtain them from radium. Then, if we should do this, who shall say

but that in time the physicist may realize the dream of the alchemist, the transmutation of the elements.

The picture of the atom as a planetary system is not without its difficulties. In the first place, our laws of electrodynamics necessitate the conclusion that an electron when moving in a circle will send out radiation continually. You will recall that I spoke of the fact that the tubes of force of an accelerated electron do not pass out radially from the electron, but suffer a contortion, which travels out into space. If the electron is accelerated first in one direction and then in another the tubes will obviously be contorted first in one sense and then in the opposite sense, so that the contortions will travel out into space in the form of waves; and, it is a well-known principle of electrodynamics that such waves carry energy with them. Now an electron moving in a circle experiences an acceleration which is directed towards the centre at each instant; and it is a comparatively easy matter to see that, as a result, it emits radiation continually. In doing so, it should gradually lose its kinetic energy; and, under these conditions would cease to move in the circle, and would describe a spiral, eventually falling into the nucleus, just as our earth would fall into the sun were it to be gradually deprived of that velocity which it has, and which enables it to keep away from the sun as a stone swung around at the end of a piece of elastic stretches the elastic in its endeavor to keep away from the centre about which it swings. Not only this, but the wave-length of the light emitted by the electron would alter as the electron spiralled in towards the centre; for the speed of revolution and the number of waves emitted per second depend on the radius of the orbit; and instead of obtaining in the spectrum of the gas a sharp line indicating a definite wave-length of light emission, we should obtain a band covering a wide range of wave-lengths.

Another difficulty following from too close an analogy between the atom and a planetary system is this. If a planet has little kinetic energy it will revolve very close to the sun, and if it has a large amount it will revolve far away, but there is nothing to lead us to suppose that it could not revolve at any distance provided that it had the proper energy. In the case of the atom, therefore, we should expect to find electrons revolving at all sorts

of distances from the nucleus, and emitting, therefore, light of all sorts of wave-lengths, instead of comparatively few.

Now Professor Bohr has endeavored to avoid these difficulties by the aid of a few extra assumptions. These assumptions are rather drastic in type, but they are very simple in form and few in number. I can best illustrate them by considering the case of the hydrogen atom. According to Bohr, the hydrogen atom consists of a single electron revolving about a nucleus of positive electricity, the proton; and in the normal state of the atom it revolves at a distance of about one-half of the one hundred-thousand-millionth of a centimetre. In order to obtain a picture of the hydrogen atom under these conditions we may say that it would look like what the earth and sun would look like if, leaving them at their present distance apart, we should squeeze the sun until it was two miles in radius. You see we cannot draw a true diagram of the hydrogen atom; for, if we should draw the nucleus sufficiently large to be seen, we should have to draw the electron bigger than the whole page, and place it on a circle 300 miles in radius.

Now the first assumption made by Bohr is that, although we might be able to knock the electron out of this ring by bombarding it with some other electron, or by some other means, we could not cause it to revolve in any ring we chose. There are only certain orbits in the atom which the electron may be permitted to have, and these are related to each other in a manner which is quite simple of expression, even though it may be rather artificial in statement. Suppose we draw a line from the nucleus to the electron, and measure the area which the line sweeps out per second. The area will, of course, increase with the size of the orbit. Now Bohr's first assumption amounts to supposing that the orbits must be such that these areas are integer multiples of the area for the smallest. We can have an orbit which makes this area twice, three times or four times the area for the smallest orbit, but we cannot have an orbit for which the area is two and a half times that unit. Moreover, the smallest orbit is such that the area described by the radius vector per second, when multiplied by 4π times the mass of the electron gives a certain number h, which first made its appearance in the theory of heat radiation and has since invaded first one branch of physics and then another

until it has finally assumed a status comparable with those of the electronic charge and mass.

In each of the possible orbits the electron possesses a perfectly definite energy, calculable in terms of the radius of the orbit according to exactly the same principles available for the calculation of the energy associated with the rotation of a planet around the sun. The greater the size of the orbit the greater the energy. We can only persuade an electron to leave the orbit in which it happens to be revolving at the time and go into some larger orbit by giving it energy; and, if it should go from a larger orbit to a smaller one, it would have to part with energy. Now the second assumption which Bohr makes is that, so long as the electron remains in any one orbit, it does not radiate any energy at all. The laws of classical electrodynamics would call for such radiation, but Bohr assumes that, for some reason or other, radiation does not take place. On the other hand, he assumes that if one of these electrons, after having been thrown into one of the outer orbits, returns to the orbit from which it was thrown, or to some intermediate orbit, it gives out the whole of its surplus energy in the form of vibrations of definite wave-length, determined entirely by the amount of energy which has been radiated. He assumes in fact that the number of vibrations emitted per second is just proportional to the energy change, the factor by which we must multiply that frequency (or number of vibrations per second) in order to obtain the energy change being the same mysterious constant h, which had already been used in specifying the possible orbits, and which as I have already pointed out had previously made its appearance in other branches of physics.

The passage of an electron from any one orbit to a smaller orbit thus gives rise to a perfectly definite wave-length radiated; and, in the ideal case, by properly stimulating the electron by throwing it out to the various orbits it should be possible to cause the atom to emit as many lines as there are possibilities of this kind of passing from one orbit to a smaller one. We thus see how it may come about that this very simple structure, consisting of no more than a positive nucleus and an electron, can give rise to a large number of spectrum lines.

Of course, it is to be distinctly understood that Bohr's theory makes no attempt to give what, in the ordinary use of words, we might call a reason for the performances which are postulated,

and it does not describe any mechanism by which the radiation is emitted during the passage from one orbit to another. I intend to elaborate rather more fully on this aspect of the matter towards the end of the paper, so will do no more at present than merely call attention to it.

Even a superficial view of the spectrum of hydrogen will show that there are orderly relations between the wave-lengths of the various lines. These relations have been studied very carefully by the spectroscopist; and, long before he had any theoretical reason for it, he knew that he could obtain the wave-lengths, λ, of a set of those lines by substituting successively the numbers 3, 4, 5, etc., for n in the formula

$$\frac{1}{\lambda} = 109{,}678\left[\frac{1}{4} - \frac{1}{n^2}\right]. \qquad (1)$$

Now the different lines which Bohr's theory predicts as corresponding to the passage of an electron between the various orbits are just those which would be obtained by substituting the numbers 1, 2, 3, etc., for n_1 and n_2 in a formula of the type

$$\frac{1}{\lambda} = 109{,}678\left[\frac{1}{n_1^2} - \frac{1}{n^2}\right]. \qquad (2)$$

A special case of this is the formula (1), where $n_1 = 2$. This is the Balmer series. But Bohr's theory predicted not only this possibility but also the possibilities to be obtained by putting $n_1 = 1$, and constructing a series on this basis, and the possibilities by putting $n_1 = 3$ and constructing a series on this basis, and so on. Some of the lines and series predicted in this way were known before the time of Bohr's theory, and others predicted by the theory have been found since. It is, moreover, a particularly cogent argument for the theory that the number 109,678, obtainable from spectroscopic data, is correctly predicted by the theory, at least to the degree of accuracy with which the quantities like the electronic charge and mass occurring in the expression of it are known.

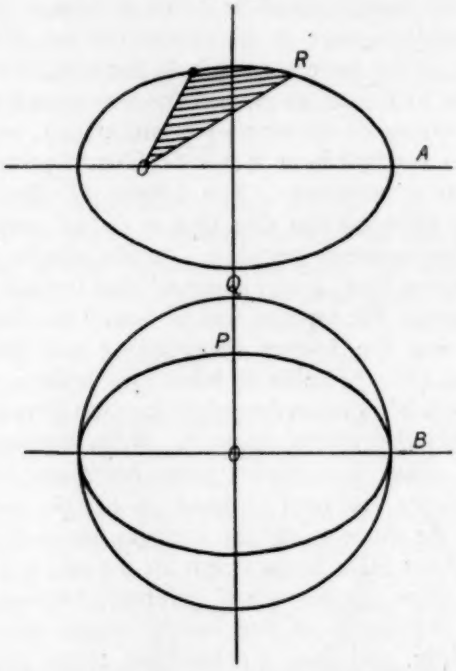
Suppose now we pass to the next heaviest element, helium. What has the Bohr theory to say? According to the views we have already discussed, to the effect that the nuclear charge changes by one unit at a time as we proceed along the periodic table, the helium atom should contain a nucleus whose total charge is twice that of the hydrogen atom; and, around it we should have two

electrons circulating in orbits. Here again, Bohr assumes that there are only certain definite orbits which the electron can describe, but the complexities of the motions for even this case are so great that the problem has not yet reached a final solution. If the helium is subjected to a strong electric discharge, however, we have reason to believe that an appreciable number of the atoms lose one of their electrons; and, under this condition, the system which remains should be just like the hydrogen atom, except that its nuclear charge would be twice as great. Bohr's theory becomes perfectly specific in its statements for this case. The corresponding orbits have exactly half the radii that they had in the case of the hydrogen atom, and the theory again predicts for the lines, a formula of the type of equation (2), with a constant differing from 109,678 by a factor of four, a result in beautiful harmony with experiment. The beauty of this harmony is enhanced still more by the fact that it is not quite exact. Let me explain this apparent paradox. In the simple theory which leads to the factor four, it was assumed that in both the hydrogen and helium atoms, the nucleus was so heavy in comparison with the electron, that the former remained at rest while the latter revolved around it. Of course, when two bodies, A and B, are of equal mass, it is no more correct to say that A revolves about B than to say that B revolves about A. What happens is that they both revolve about a common point half-way between them. When the masses are very unequal, as in the case of the sun and earth, or the nucleus and the electron, the point about which the rotation takes place is very near to the centre of the heavier body. If we allow for the actual difference between the point of rotation and the centre of the heavier mass, our formulæ are modified slightly, and they are modified to an extent which is different for helium and hydrogen, since the helium nucleus is heavier than the hydrogen nucleus. The result of this is that the numerical factor which occurs in the formula for helium should not be just four times 109,678.3 but 4.001626 times that number, and this is just what it is.

Before leaving the hydrogen atom, I must refer to one or two other matters of very fundamental importance. You will observe that so far we have always spoken of the orbit of the electron about the nucleus as though it were a circle. We know, however, that both from theory and from observation, the orbit of a planet

around the sun is, in general, an ellipse, with the sun at one focus; and, only in very special cases does this ellipse degenerate into a circle. Our atom would, therefore, seem much less artificial if we could permit the orbits to be ellipses. You will recall that the possible circular orbits were determined by the law that they were such that if the area swept out per second by the electron in the innermost orbit was $h/4\pi m$, in the next orbit it was twice this

FIG. 5.



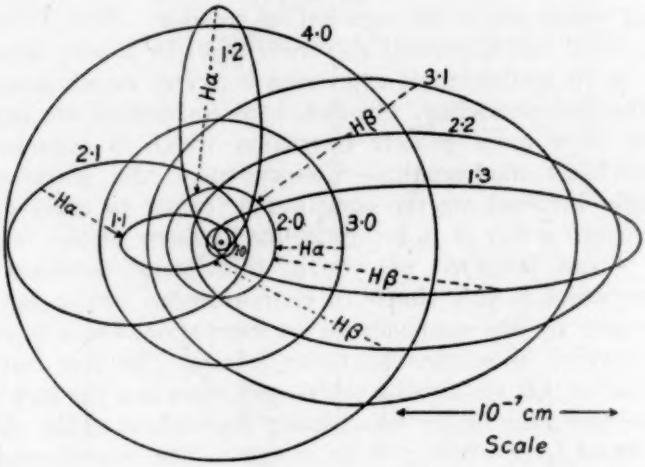
amount, and so on. Now the laws of nature have been very kind to the mathematician in providing for the well-known fact that, in the case of elliptic motion about a centre of force, Fig. 5, *A*, equal areas are swept out by the radius vector in equal intervals of time, so that if we speak of the area swept out per unit of time this has a definite meaning wherever the electron happens to be in its orbit. Unfortunately, however, while to fix the area swept out per second by the radius vector completely determines the orbit in the case of a circular orbit, it does not do so in the case of the elliptic orbit, since, for any given value of this quantity, there

are an infinite number of different degrees of ovalness, or eccentricity as the mathematician calls it, which the orbit can have; and, in each of these orbits the electron possesses a different energy. If we should try to build a theory on the lines successful for the circular orbits, we should immediately land ourselves into trouble, because the passage of the electron from any one of the infinitely many orbits to any other would correspond to a line in the spectrum, which was different from that corresponding to the passage between any other pair of orbits, so that we should predict spectra with infinite numbers of lines instead of finite numbers as experiment requires. If we are to have elliptic orbits, we must find some further restriction to impose upon them in order to limit the number which are to be regarded as possible. Now Professor Sommerfeld has expressed this restriction in a very beautiful way. In its mathematical expression it is very closely bound up with the first restriction. In fact, both restrictions are separate aspects of a more general restriction which is expressed in Sommerfeld's mathematics. Unfortunately, the mathematical principles involved are too complicated for me to express them here in such a way as to bring out the harmony of this relationship. I can, however, tell you of this second condition in a manner which is very simple of expression but which, as a sort of penance for the simplicity of its expression, hides from you the beauty of its relationship to its fellow. The first condition is similar to that for circular orbits, and states that the area swept out per unit time by the line joining the nucleus to the electron must be $\tau h/4\pi m$ where τ is an integer. The second condition, which serves to fix the ovalness of the orbit, amounts to this. Suppose we consider the elliptic orbit in question, characterized by the integer τ as regards the area swept out per second. Let us draw a circle about the ellipse in such a way as to just inclose the major axis. Then, the ellipse must be such that the ratio of QP to PO, Fig. 5, B, is the ratio of some integer number to the number . With this restriction we limit ourselves to a finite number of possible orbits. There are many more than in the case of the circular orbits; but, for a curious reason, resulting from the fact that they do not all correspond to different energies, the situation is not greatly complicated thereby. Fig. 6 represents the possible orbits for the case of hydrogen. It is customary to speak of each of these orbits as corresponding to a certain energy

level, meaning thereby the energy of the electron when in the orbit. Now corresponding to the lowest energy level we find only one orbit, a circle, and the same circle as we encountered in the simple theory which confined itself to circular orbits entirely. Corresponding to the next energy level, which is again the same as in the case of purely circular orbits, we find two orbits, a circle and an ellipse. Corresponding to the next energy level, which is the same as the third level for purely circular orbits, we find one circle and two ellipses, and so on.

Carrying over the assumptions of the original Bohr theory, we

FIG. 6.



have to recognize that whenever an electron passes from one energy level to a lower one, it emits radiation of a frequency determined by the difference of the energies. Now the number of different energy levels is no more than it was on the simple theory of circular orbits. It is true that any given energy level may be realized in several ways, in an ellipse of one or of another kind, or in a circle; but, so long as we believe the Bohr hypothesis, any pair of orbits is equivalent to any other pair having the same corresponding energies, as regards the frequency of the light emitted when an electron passes from one member of the pair to the other. We thus see how this extension to elliptic orbits, while greatly enhancing the logical beauty of the scheme, is not attended

by a destruction of the valuable features presented by the simpler model with circular orbits.

Once again, however, the foundations of the more complete theory receive a more complete justification from the fact that they are not quite correct. The matter of the relation between the numerical factors in the formula for the hydrogen and helium lines becomes satisfactorily taken account of, as in the simpler model for circular orbits, by recognition of the fact that the nucleus does not remain absolutely fixed during the revolution of the electron; but, there is something else. Electrodynamics and the theory of relativity alike support the conclusion that the mass of an electron experiences a very slight increase as the velocity of the electron increases. This increase mounts to an infinite extent for an electron which approaches the velocity of light; but, for velocities as great as twenty miles per second, it only amounts to one-half of 1 per cent. The velocities of the electrons in their orbits are sufficiently great to cause this variation of mass with velocity to play a small part in the dynamics of the system; and, the important thing which results is this; if we pick out all those elliptical orbits which on the basis of neglect of change of mass with velocity corresponded to the same energy level, these orbits will, with the change of mass with velocity taken into account, have very slightly different energy levels. Now what will be the result of this? Let us concentrate our attention on the two orbits, (2·0) and (1·1), which in the absence of this relativity correction had next to the lowest energy level, and the three orbits, (3·0), (2·1), and (1·2), which under that condition had the energy level next but one to the lowest. In the absence of the relativity considerations, the frequency emitted by an electron was the same in a fall from (3·0) to (2·0) as from (2·1) to (2·0) or (1·2) to (2·0), or from (3·0), (2·1), or (1·2) to (1·1). With the relativity correction taken into account, however, the energies corresponding to (3·0), (2·1) and (1·2) will be slightly different from one another, and the energies corresponding to (2·0) and (1·1) will be slightly different from each other, so that there will be a slightly different change in energy in falling from (3·0) to (2·0), from what there will be in falling from (3·0) to (1·1), for example. Where, on the simple view, we got but one line in our spectrum corresponding to the fall of an electron between these two energy levels, we now get several, differing from each other

by very small amounts. The theory thus leads us to believe that if we should examine the lines in the spectrum of hydrogen with sufficient care we should find that they consist of a number of separate lines very close together. This is exactly what experiment reveals; and, the measurement of the small separations of these lines, and their comparisons with the predicted amounts, have resulted in the most beautiful verifications, not only of the Bohr-Sommerfeld theory, but also in the conclusions which the theory of relativity has suggested for the change of the mass of the electron with velocity. I might go on to show how the Bohr-Sommerfeld theory accounts for those remarkable effects produced on the spectrum of hydrogen by intense electric and magnetic fields, but time requires that I should pass on to other things.

The question which naturally suggests itself is, what becomes of the theory when applied to atoms more complicated than hydrogen and helium? Here we meet with problems of great complexity. We are encouraged, however, by the fact that the spectra of even the heaviest atoms do not exhibit an infinite degree of complexity, so that in order to account for the comparative simplicity which is found, nature must have held herself within bounds, and must not have striven too hard to present to poor man problems which would baffle him completely. The ideas applicable to the simple atoms have been extended by Bohr to the more complex, and briefly the situation is this. We have in each atom a number of orbits which the electrons can describe, each corresponding to a definite energy level. In the normal state, the electrons occupy certain of these orbits, there being one orbit for each electron. In the simplest atom, hydrogen, this orbit is the innermost orbit, but in the more complex atoms, orbits which are not the innermost may be occupied by the electrons. By subjecting the atom to an external stimulus as by shooting a rapidly moving electron into it, or by violent temperature agitation, electrons in the atom may be removed from the orbits which they originally occupied and caused to move in others in which they move with higher energy, but from which they tend to fall back as soon as possible to the normal state—not necessarily in one jump, but occasionally by occupying intermediate orbits for a short time in the process of their return to the normal state. In the process of passing from any one orbit to an orbit of lower energy, the surplus energy is given up in the form of a radiation of a perfectly definite

wave-length characteristic of the energy change which has taken place. Electrons which are removed from inner orbits of heavy atoms, and fall back to such orbits, give out a large amount of energy in so doing; the waves which they emit are very short, and constitute the X-rays. By discussion of the dynamical properties of the various orbits, Bohr has endeavored to connect the properties with those to be expected from chemical considerations, but the story is too long and involved for the present paper.

You have probably been struck by the artificiality of the laws which govern the atom, of their arbitrary nature. Take, for example, the law which states that the only ellipses which the electron can describe are those for which a certain area is an integer multiple of a certain prescribed unit, and which also satisfies the other rather curious condition which I have specified. The more conservative physicist will feel inclined to exclaim, "These are no laws at all, but simply a system of rules; and, the whole thing cannot be dignified by the name Theory until some explanation of these rules has been found, some explanation in which the assumptions appeal to our sense of reason." But wherein lies the origin of our conviction that certain laws are reasonable and others unreasonable and meaningless? Is it not founded in part upon our more practical familiarity with the phenomena whose laws we are pleased to call reasonable? We who have pondered for so many generations on the laws of matter in bulk, and are now suddenly confronted with the laws of the atom, are we not to some extent in the position of a giant who, having looked down for a generation upon a great city and studied its ways, is suddenly brought to a realization of the fact that there is such a thing as a brick which he must also study? What will be the processes of thought of our giant? In his study of the city, he has noted that it has a mayor, a police department, a fire department, traffic signals, and so forth. These things have become very familiar to him, and he will feel that he understands anything which works that way. What then will he say about the brick? How will he explain to his satisfaction, its tenacity, its heat-resisting properties and the like? Will he not seek, in the brick, a mayor, a police department, a fire department and traffic signals in terms of which to correlate his thoughts, and will he not grumble if anyone wants to discuss the brick in any other lan-

guage? We laugh at this, but is not this our own attitude if we expect the laws of the atom to be of the same type as those for matter in bulk? How shall we expect to explain the atom itself in terms of the more coarse-grained properties of matter which we expect the atom itself to explain?

I think that it is safe to say that the more we ponder over this question of what is reasonable and what is unreasonable the more we come to the viewpoint that the criteria of reasonableness are to some extent arbitrary, and functions of that field of nature's phenomena which happen to have been studied by our immediate predecessors. An astronomer of the time of Galileo, a man of some eminence, argued thus against Galileo's discovery of the eighth planet: "There are seven openings in the head, two nostrils, two eyes, two ears and a mouth. There are seven days in the week. There are seven colors in the rainbow," and he found quite a number of other sevens. "Therefore," he argues, "if there be an eighth planet, all of this is meaningless." Of course we laugh at him; but, we must realize that this mediæval astronomer was probably no fool as scholars go. He had, however, a criterion of reasonableness, which in our time appears very distorted. It was one which would not have carried him very far in correlating the facts as we know them to-day. It did express a few facts. There *are* seven days in the week, seven openings in the head, and so forth; and if our friend had probed the matter far enough, he might even have predicted something about the periodic table of elements.

For the last three hundred years we have grown up in the doctrine of Newton. We think of forces and inertia. A body moves in a straight line with a constant velocity, and we say there is no force on it; it travels otherwise, and we say there is a force acting on it. This is no mysterious law of nature, but rather a statement of what we intend to *mean* by the absence or presence of force; and, the measure of force as mass times acceleration is a definition rather than a law of nature. Its usefulness lies in the fact that the force as so defined has very simple properties in astronomy, being, in fact, a function only of the positions of the bodies exerting the force, and not of their velocities. We may well ask ourselves occasionally why it is that, on the average, the statements of our physical laws are as simple as they are. How far is this evidence of some profound tendency on the part

of nature to be simple, and how far does it arise from the fact that mankind, being unconsciously wise, has chosen to give names to those things which happen to have simple properties. The product of mass and velocity of a body has a simple property, so we give it a name "momentum"; and, after we have used it and thought about it for a hundred years or so it becomes a very real thing to us. Again, one-half of the square of the velocity of a body multiplied by its mass has a simple property, and it is granted a name, the kinetic energy. The product of the cube of the velocity and the mass has no simple property, so it gets no name. We say it doesn't mean anything in particular; and any theory which gave it a prominent place would be classed as artificial and possibly meaningless.

The tendency of the most radical thinkers of the day is to cease thinking so much of "reasons," and be concerned mainly with the expression of the facts. This is by no means as barren a field of thought as you might suppose. In the great realms of mathematics there are oases of great beauty, in which many harmonies find expression. Each of these represents a sort of universe which might have been. It is the problem of the mathematical physicist to seek among these one which duplicates nature's laws as we find them. One of these oases is the general theory of relativity, a realm in which the language is quite different from that to which we have accustomed ourselves in the past, but one more satisfying to the mathematician who sees in the beauties of a mathematical curve a more fit field for the expression of nature's laws than in the primitive concepts of push and pull. We may gather some conception of the mathematician as he surveys the possibilities of such a realm as that of relativity by pondering upon the words of Einstein, its greatest exponent, who has likened it to a most beautiful and wonderful symphony of which we can but say that time alone will show whether it is the symphony which God has played.

Smoke Abatement.—The problems involved in this important subject have been recently presented by the U. S. Bureau of Mines, in a pamphlet (Tech. Paper, No. 273) prepared by OSBORN MONNETT. The enormous development of industrial operations has rendered this unpleasant condition very common, cities of the eastern part of the country that for many years used anthracite almost exclu-

sively, have been forced to the use of the softer coals, even in some cases for domestic heating. Five-sixths of the coal mined in the United States to-day is bituminous. As is well known, the great difficulty with such coal is that it begins to lose heavy hydrocarbons before enough air can be introduced into the mass. That much can be done in manufacturing establishments by installing proper feeding apparatus has been amply shown by experiment and actual operation of plants. Many investigations of the subject have been made, one of the most comprehensive among recent ones is that of the Chicago Chamber of Commerce, which, however, finds many difficulties in estimating the damage done to persons, property and vegetation. The general conclusion is that health both of animals and plants is injured. The injury to property is not only due to disfigurement, but to the sulphur oxides emitted by the combustion. It must, of course, be borne in mind that this form of injury will not be in any way abated by the most complete removal of the smoke nuisance, since the sulphur will be present in as great a quantity as ever. Sulphur is, indeed, one of the worst "black beasts" with which the industrial engineer has to deal.

The bulletin gives much information as to the management of fires using soft coal, especially for domestic purposes, the descriptions being made quite vivid and serviceable by numerous illustrations. Attention is given to the use of coke. This seems to be the most promising line of correction of the smoke evil, for if by-product coke-ovens are extensively installed, a large amount of valuable hydrocarbons will be available for many industrial purposes, including liquid fuel, and coke might be brought to a price competing with anthracite, thus giving a smokeless fuel convenient to handle. Two classes of coke are now recognized: The high temperature product, obtained by operating at about 1000° C. and the low temperature product for which the retorts are not heated above 600° C. Low temperature coke contains considerable volatile matter which causes it to ignite readily, having somewhat the quality of a semibituminous, which is a well-known excellent steam fuel. The tar obtained is a thin liquid, perhaps better termed an oil, and is rich in the hydrocarbons that are useful in motor fuels. The amount of permanent gas yielded is smaller than that furnished at the high temperature, but the heating value is much higher. One ton of coal carbonized at not over 600° C. will yield about as follows: 1400 pounds of coke, 2000 to 7000 cubic feet of gas, 20 to 30 gallons of tar, 2 to 8 pounds of ammonium sulphate. Extensive studies of the low temperature methods were made by the Bureau of Mines and the results are given in the bulletin.

Among the possible methods of relief from the smoke nuisance is the use of colloidal fuel, that is fine coal suspended in crude oil and the plan of burning the fuel at the mines and transmitting power from there. The so-called "Giant Power" scheme will have an important bearing in the matter. H. L.

MEASURING OCEAN DEPTHS BY ACOUSTICAL METHODS.*

BY

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THE possibility of measuring ocean depths by acoustical methods has been recognized for a number of years and numerous methods and devices developed and designed for this purpose are listed in the patent office. Most of these devices attempt to determine the depth in terms of the time required for a sound signal to travel to the sea-bottom and reflect back again to the surface.

One of the first patents pertaining to this art was granted to A. F. Eells, of Boston, Massachusetts, wherein he was allowed two broad claims covering the method of determining depths by measuring the time intervening between the transmitting of a sound signal near the sea-surface and the return of its echo from the sea-bottom. Since then numerous patents have been taken out covering specific apparatus designed for measuring this time interval, but none of these devices has proved to be practical for the reason that they have failed in most cases to measure the time interval in question with sufficient reliability and accuracy, and in many cases have proved to be too delicate to withstand the adverse conditions often met with on sea-going vessels and too complicated to be operated by a ship's personnel. A brief description of some of these devices follows:

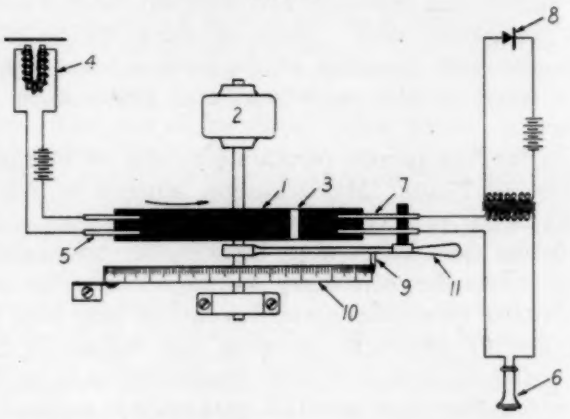
Fig. 1 shows the principle of operation of a sounding device invented by Reginald A. Fessenden.[1] Numeral 1 represents a disc made of insulating material that is rotated at a uniform speed by motor (2). The disc carries a conducting segment (3) that closes the electrical circuit through a submarine sound transmitter (4) when it passes beneath brushes (5), thereby sending out a sound signal. This segment also closes the circuit through

* Presented at the Stated Meeting of the Institute held Wednesday, March 21, 1923.

[1] For a more complete description of the Fessenden depth-sounding apparatus, see U. S. Patent No. 1,217,585.

a telephone receiver (6) when it passes across the brushes (7). If the echo of the signal meets the microphone or other type of sound receiver represented by numeral 8 at the instant segment (3) short-circuits brushes (7), it will be heard in the telephone receiver (6) and the time of sound transmit from the transmitter to the receiver by way of reflection from the sea-bottom will be equal to the time required for segment (3) to travel the angular distance subtended between the two pairs of brushes and indicated by the pointer (9) on the scale (10). This condition is brought

FIG. 1.



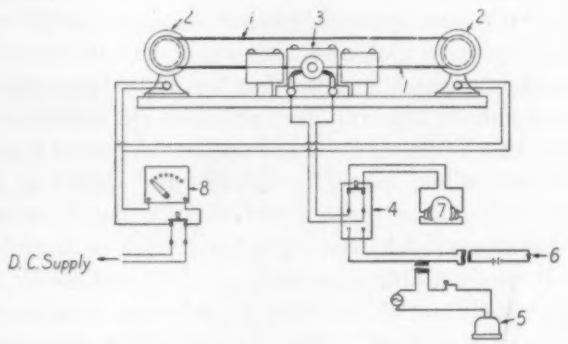
about by rotating brushes (7) about the insulating disc (1) by means of the handle (11).

In practice the disc must be rotated at considerable speed or the angle swept out by the segment while the sound travels to the sea-bottom and back will be too small to measure with sufficient accuracy. This results in sending out sound signals in rapid succession and the return of echoes from the sea-bottom in still more rapid succession for the reason that a sound signal usually echoes back and forth between the surface and sea-bottom several times before its energy is absorbed. Under such conditions sound can be heard in the telephone for numerous settings of the brushes (7) and the relation between the depth and the scale reading becomes indefinite.

Another device for measuring this time interval makes use of an electromagnetic recorder. This device, illustrated in principle

in Fig. 2, attempts to determine the short-time intervals involved in taking shallow soundings by recording the transmitted signal and its returning echo on the magnetic tape (1), while it is driven rapidly by means of variable speed motors (2), and then measuring the time interval between the two records when the tape is run at a much slower speed. This measured time interval multiplied by the ratio of the reduced speed to the recording speed gives the time interval between signal and echo. In the figure, numeral 3 represents the recording and reproducing magnets. These also serve for erasing the record. Numeral 4 represents a double-

FIG. 2.



throw double-pole switch by means of which magnets (3) can be inductively connected with transmitter (5) and receiver (6) for recording the signal and its echo, or with the telephone head set (7) for hearing the reproduced record. The rheostat for controlling the speed of the motors is indicated by numeral 8.

This method, while excellent from the standpoint of theory, has not proved to be practical for the following reasons:

(*a*) The magnetic tape does not record the signals unless their intensity is above a certain threshold value, which is comparatively high, and the echoes cannot be kept above this value over regions where the coefficient of reflection of the sea-bottom is low or over regions where the depth is great.

(*b*) The local disturbing noises always present on shipboard are comparatively intense and their record ofttimes distorts the record of the signals and echoes to such an extent that they cannot

be readily recognized, and as a result the time interval cannot be accurately measured.

(*c*) It is difficult to determine accurately the ratio between the reproducing and recording speeds of the motor.

(*d*) The time interval, as measured at the retarded speed, cannot be determined with a high degree of accuracy for the reason that the record of both the signal and the echo then becomes less sharply defined; and although this method of determining the short interval between signal and echo results in some gain in accuracy, the gain is not proportional to the ratio between the recording and reproducing speeds.

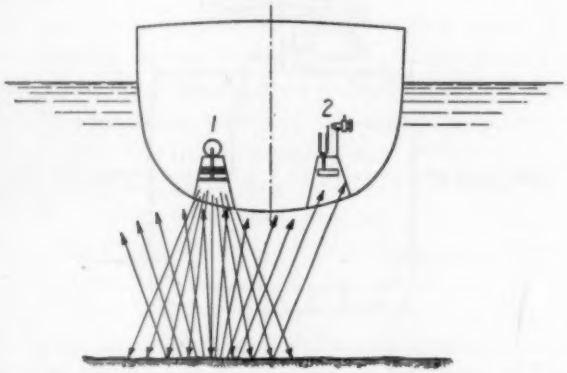
(*e*) Finally the method is too slow to give soundings on short notice, as is ofttimes desirable when a ship is in dangerous waters.

Samuel Spitz, of Oakland, California, has attempted to measure ocean depths with apparatus that first records the signal and its echo on a magnetic tape and then amplifies the reproduced record. He utilizes this increased electrical output to operate a complicated system of relays and magnetic clutches and claims to accurately record by means of a pointer and dial the depth corresponding to the time interval between signal and echo as recorded on the tape. His method and apparatus, as disclosed in U. S. Patent 1,409,794, would appear to have the inherent weaknesses of the "magnetic tape method" plus the difficulties and uncertainties that are always present to a greater or less degree in complicated relay systems. But even if the relays should function perfectly, it would seem that the local disturbing noises, caused by propellers, auxiliary machinery and slapping of waves, which (no matter how selective the receiving system may be) are recorded on the magnetic tape to some extent, would ofttimes trigger off the automatic recording apparatus and give erroneous records of depth that might be misleading. A depth-sounding device is worse than useless unless it can be absolutely depended upon to give reliable sounding data at all times.

Alexander Behm, of Kiel, Germany, started work on the problem of measuring ocean depths by means of sound waves about twelve years ago. His first efforts were devoted to methods that involved measuring the time interval between signal and echo. Recognizing the inherent difficulties in making such measurements, he turned his attention to the possibility of making depth

determinations by measuring the intensity of the echo. His method of doing this can be understood in connection with Fig. 3 wherein numeral 1 represents a submarine sound transmitter designed to produce a fairly pure sound of constant intensity and pitch. The sound passes from the transmitter to the sea-bottom and a portion reflects back to the receiver, represented by numeral 2, where by acting upon a resonant chamber it causes a tuning fork to vibrate. He measures the intensity of the echo in terms of the amplitude of vibration of a small bead carried by one prong of the fork and observes the amplitude of its motion by means of a microscope. And since the intensity of the echo and

FIG. 3.

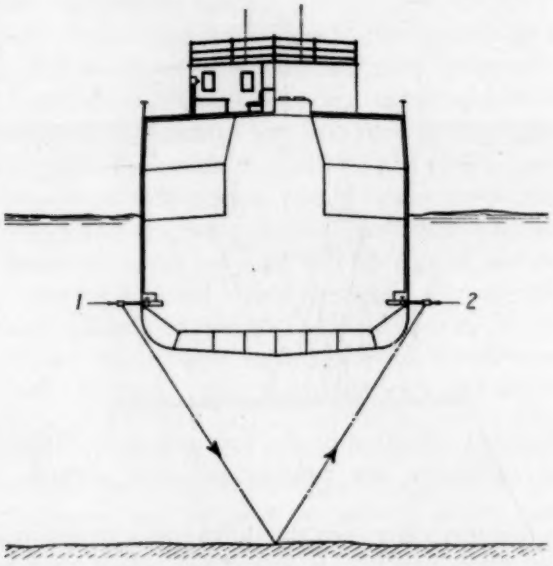


the amplitude of vibration of the fork are each a function of the depth, he calibrates the microscope scale directly in terms of depth.

While this method avoids the difficulties encountered in measuring short time intervals, it introduces others that are equally hard to overcome and one that cannot be overcome. It is very difficult to generate a sound having constant intensity and pitch under various operating conditions, and it is equally difficult to keep a receiver accurately tuned to this pitch and of unvarying sensitivity. It is probable that Doctor Behm has gone far toward overcoming these two difficulties, but we fail to see how he can make allowance for the variations of the coefficient of reflection of the sea-bottom where, according to our observations, this factor may change as much as 25 per cent. over comparatively short distances.

It is probable that Doctor Behm fully recognized these weaknesses in his method and apparatus for he later resumed his efforts to measure the time between signal and echo and has finally succeeded in making this measurement with a high degree of accuracy with comparatively simple and rugged apparatus. His transmitter, represented by numeral 1 of Fig. 4, consists of a tube extended through the ship's skin and the sound signal is produced by exploding a cartridge that has been slipped into position in this tube. The cartridge is fired electrically by closing a key mounted

FIG. 4.



on or near the recording apparatus which is located in the chart house or on the bridge. The receiver is mounted in a similar tube, numeral 2, projecting from the opposite side of the ship's hull where it is shielded from the direct sound generated by the cartridge. The means for recording the time of sound transit between transmitter and receiver by way of reflection from the sea-bottom consists of an ingenious design of chronograph that is started moving when the cartridge is fired and stopped by the fluctuation in the receiver circuit when the sound wave, reflected from the sea-bottom, strikes the receiver.

This sounding device, which the inventor calls the Behm-Echolot, represents a large amount of excellent research and the exercise of considerable ingenuity. It should give reliable sounding data for depths within about eighty fathoms.

As an instrument for aiding navigation, any depth-sounding device should be able to do more than determine the depth of water occasionally. It should serve to take any number of soundings in rapid succession in order that well-defined charted contours may be identified with certainty and thus serve for determining the position of the vessel. In case of the Behm-Echolot this would not only require a very large supply of cartridges, but would prove to be expensive.

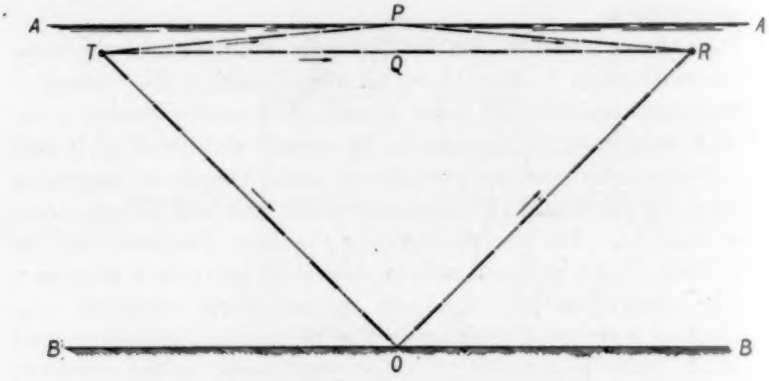
It has been found in practice that a determination of the slope of the sea-bottom is helpful in locating "landmarks" along a charted route and that the value of a depth-sounding device, as an aid and safeguard to navigation, is greatly enhanced if it will also serve to determine the direction of sound beacons at dangerous points along shore and at harbor entrances, and also signals from other vessels. The depth-sounding devices developed in the U. S. Navy, and which will now be described, serve these purposes.

The determination of depth by acoustical methods was assigned as a research problem in the Bureau of Engineering of the U. S. Navy as a result of a discovery made on the transport *U. S. S. Von Steuben* during a trip from New York to Brest in March, 1919. This vessel had been equipped with one of the submarine sound receivers that the Navy had developed at its New London Station for use in locating U-boats and it was proposed to test the value of the device as an aid and safeguard to navigation. It was found that the direction of nearby vessels and submarine bell signals could be accurately determined while leaving New York harbor and when approaching Brest, but that in mid-ocean the propeller sounds of other vessels as well as of the *Von Steuben* herself could not be heard. This fact led to the discovery that the only sounds heard in a submarine sound receiver located near the surface are the components that have been reflected from the sea-bottom.

The explanation of this fact can be readily understood by referring to Fig. 5 wherein $(A-A)$ represents the sea-surface, $(B-B)$ the sea-bottom, and T and R a sound transmitter

and a sound receiver, respectively, each submerged a distance represented by $(P-Q)$. If the distance $(P-Q)$ is small compared with the distance $(T-R)$ (as is usually true in practice), then the two sound paths $(T-P-R)$ and $(T-Q-R)$ are practically of equal length. Sound from T reaches R by the three paths $(T-P-R)$, $(T-Q-R)$, and $(T-O-R)$, but since the two paths $(T-P-R)$ and $(T-Q-R)$ are practically equal and since the surface-reflected ray suffers a change of phase of a half a wave-length upon reflection, the sound traversing these two

FIG. 5.



Mutual cancellation of direct and surface-reflected rays.

paths interferes destructively at R and only the sound that has been reflected from the sea-bottom can be heard.

As soon as it was discovered that the sounds heard in our receivers had arrived by way of reflection from the sea-bottom, it appeared probable that methods could be developed for determining the depth of the water by means of submarine sound waves, providing the character of the sea-bottom in general is such as to reflect sufficient sound energy to give an audible echo. Before starting this work some preliminary tests on the efficiency of the deep-sea floor as a sound reflector were made on the destroyer *U. S. S. Wilks* which showed that clear audible echoes of signals from submarine sound oscillators could be received from depths at least as great as 2000 fathoms. Since that time good echoes have been received in depths greater than 3000 fathoms and so far

as the author knows there has been no case reported where echoes could not be heard.

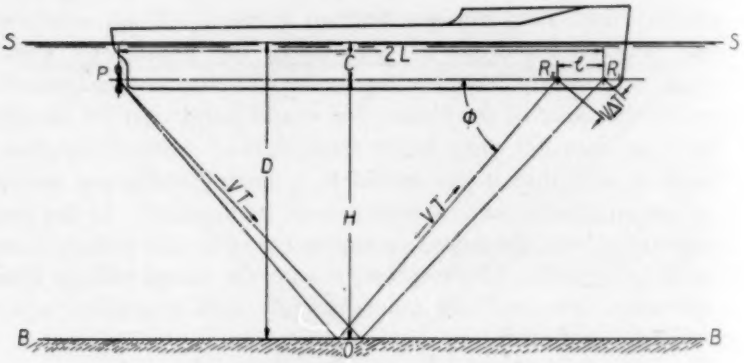
It should be stated, however, that nothing definite is known regarding the coefficient of reflection of the sea-bottom other than the fact that echoes have been heard over such regions or routes as have been tested. Over certain regions it has been noted that the echoes are much less clear-cut than are the signals. This distortion is doubtless due to a gradual change in the density of the material forming the sea-bottom. But the fact that an echo is heard at all would seem to argue against the somewhat general conception that the deep-sea bottom consists of an ooze-like deposit perhaps hundreds of feet thick, the density of which increases very slowly from the top to the rock foundation beneath. From the character of the echoes one would judge that the density of the ooze does not vary much from that of water throughout its depth or else that it has settled to a dense foundation, except for a comparatively shallow region near its surface. If the first assumption is true, the sound penerates the ooze and reflects from the underlying rock. If the second is true, the sound reflects from the solidified ooze and the comparatively thin transition layers immediately above this.

Three methods have been developed for determining ocean depths by means of sound waves, two of which serve for measuring depths less than about one hundred fathoms and one of which serves for measuring any depth greater than about forty fathoms. All three methods make use of the time required for a sound signal to travel from a transmitter to a receiver by way of reflection from the sea-bottom. It will be seen that this time interval, which is too short to be measured directly with sufficient accuracy, can be determined indirectly as a function of a much shorter time interval that can be very accurately measured. Of the two methods that serve for determining shoal depths, one has been termed the "Angle of Reflection Method" and the other the "Standing Wave Method." The method that serves for greater depths has been called the "Echo Method."

The angle of reflection method can be understood by referring to Fig. 6, wherein $(B-B)$ represents the sea-bottom (supposed for convenience to be horizontal), and $(S-S)$ represents the surface. The propeller (P) of the vessel represents a sound

source and R_1 and R_2 represent two sound receivers mounted within the peak-tank or within a blister-like enclosed space on the outside of the ship's skin. These receivers are spaced a distance (l) on a horizontal line passing through the propeller. The distance between P and the mid-point between the two receivers is $2L$. The path of the sound waves from propeller to receivers will be $(P-O-R)$. If the sea-bottom is horizontal, the triangle $(P-O-R)$ is isosceles, having the side $(P-O)$

FIG. 6.



Sounding by angle of reflection method.

equal to the side $(O-R)$. If $2T$ represents the time of sound transit from P to R, and V represents the velocity of sound in sea-water, then,

(1) $H^2 = (V.T)^2 - L^2$

(2) and $\frac{V.T}{L} = \frac{l}{V.\Delta T}$, (corresponding sides of similar triangles)

(3) wherefore $V.T = \frac{l.L}{V.\Delta T}$.

Substituting the value of $(V.T)$ in equation (1) gives

(4) $H^2 = \frac{l^2L^2}{V^2.(\Delta.T)^2} - L^2 = L^2 \left\{\frac{(l^2 - V^2.(\Delta T)^2)}{V^2.(\Delta T)^2}\right\}$

(5) wherefore $H = L.\frac{\sqrt{l^2 - V^2.(\Delta T)^2}}{V.\Delta T}$

where ΔT is the difference in the time of arrival at the two receivers of corresponding increments of the propeller sounds. The total depth (D) is given by the equation:

$$(6)\quad D = C + H = C + L \cdot \frac{\sqrt{l^2 - V^2(\Delta T)^2}}{V \cdot \Delta T}.$$

All the factors on the right-hand side of equation (6) are constant and known except ΔT. The distance the receivers and propellers are submerged is C, half the distance from the propellers (or whatever source of sound is used) is L, the spacing of the two receivers is l, and V is the velocity of sound in sea-water which may be regarded as constant. The determination of depth therefore depends upon the determination of ΔT. This time factor, though much smaller than the time interval between signal and echo, can be determined with a high degree of accuracy by making use of the so-called binaural sense.

It has been proved experimentally that the direction of sounds is largely determined by the difference in the time of arrival of corresponding portions of the sound waves at the two ears. If the sound strikes the right ear first one unconsciously judges the source to be located at his right, if it strikes the left ear first he judges the source to be located to his left. If the sound strikes both ears simultaneously it appears to be neither to the right nor left and is said to be binaurally centred. The sense of direction of sounds, which is dependent upon the difference in the time of arrival at the two ears, has come to be called the "binaural sense." When one judges the direction of a sound to be neither to his right nor left, or in other words, when he judges it to be binaurally centred, he unconsciously estimates that the sound waves strike the two ears simultaneously; and the high development of the binaural sense is such that he estimates correctly to within about one two-hundred-thousandth of a second.

Of the two receivers shown in Fig. 6, suppose the output from one is brought to one ear of the operator, and that from the other receiver is led to the other ear, respectively. If the time of energy transit to the ear from each receiver is the same, the difference in the time of arrival of the sound at the two ears will be ΔT, the time difference in arrival at the two receivers, and, through the operation of the binaural sense, the sound will appear to come from the side of the operator because one ear is stimulated earlier

than the other. Moreover, the sound will appear to be located on the side of the observer carrying the ear that receives the earlier stimulation. If now the energy-conducting path leading from each receiver to its respective ear be constructed so that the time of transit over the path can be varied continuously or by very small increments, it will be possible for the operator to make the sound reach his two ears simultaneously by increasing the time of transit across the path leading to the ear that is first stimulated or decreasing the time of transit between the other receiver and its respective ear or by increasing the time of transit to one ear and simultaneously decreasing the time of transit to the other ear by a proper amount. When the sound has been binaurally centred in this way the difference in the time of transit across the two paths, connecting between the two receivers and the observer's ears, respectively, is equal to the time increment (ΔT).

The process of binaurally centring a sound in this way has been called "compensation,"[2] and any device used for this purpose is called a "compensator." It is evident that the compensator can be calibrated to give the value of ΔT or any function of ΔT, and as a result can be calibrated to give the depth (D) directly. This is done in practice.

It is to be noticed that the last term of equation (6) can be written as the tangent of the angle (ϕ) that the direction ($O-R$) makes with the horizontal line ($P-R$) and that equation (6) can be written:

$$(7)\quad D = C + L\tan\phi.$$

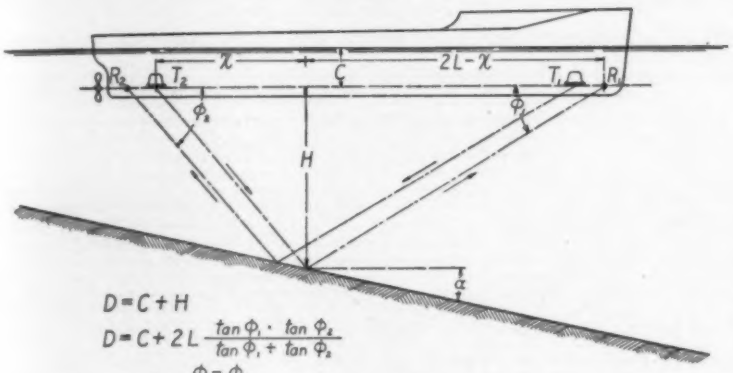
When the sounding equation is put in this form it becomes obvious that the sounding data given by the angle of reflection method become more accurate as the depth becomes less, a result much to be desired. On the other hand, it shows that the method breaks down for depths so great that the angle (ϕ) approaches a right angle. It has been found in practice that this method gives reliable soundings to depths equal to about three times the distance between the sound source and the receivers. For most

[2] The process of binaurally centring a sound by compensation was developed by the Navy in 1917–18 at its research station in New London, Conn. For a more complete description of the process, see *Proc. Amer. Phil. Soc.*, 59, No. 1, 1920, or the *Marine Rev.*, Oct., 1921.

vessels this will cover depths as great as 100 fathoms and ofttimes more.

It is evident that the method, as described, becomes inaccurate when the slope of the sea-bottom varies from the horizontal for the reason that the triangle $(P-O-R)$ will not be isosceles. In general the method gives too great values when the vessel approaches shoaling water and too small when running into

FIG. 7.



Method for eliminating error due to shelving bottom.

increasing depths. In most regions the sea-bottom within the hundred fathom curve is fairly uniform for the reason that it has been leveled off by the action of storm waves and the method described has been found to give fairly accurate sounding data.

By referring to Fig. 7 it will be seen that the method can be made accurate by installing a sound transmitter and receiver in each end of the vessel. The same compensator serves for both sets of receivers as does the same power outfit for driving both transmitters. The operator sounds transmitter (T_2) and with his compensator measures ϕ_1. Then by throwing a miltpole switch he sounds T_1 and measures ϕ_2. It can be shown that

$$(8)\quad D = C + 2L\frac{\tan\phi_1 \cdot \tan\phi_2}{\tan\phi_1 + \tan\phi_2},$$

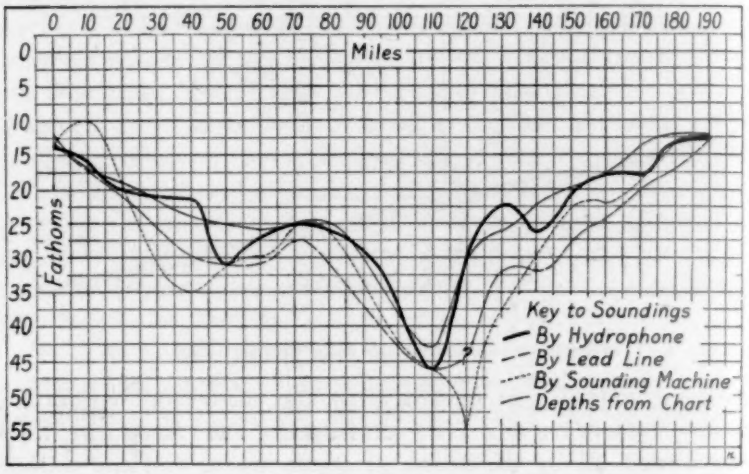
and that α, the slope of the sea-bottom, is given by the equation,

$$(9)\quad \alpha = \frac{\phi_1 - \phi_2}{2}.$$

While this more refined method is to be preferred for making hydrographic surveys, the simpler approximate method serves for navigational purposes as can be seen by a consideration of the curves shown in Figs. 8 and 9.

The curves of Fig. 8 represent sounding data taken by the *U. S. S. Breckenridge* while en route from Charleston, South Carolina, to Key West. The course ran in part along the edge of the continental plateau, where the sea-bottom was very uneven and erratic. Two successive casts of the hand-lead made not more

FIG. 8.



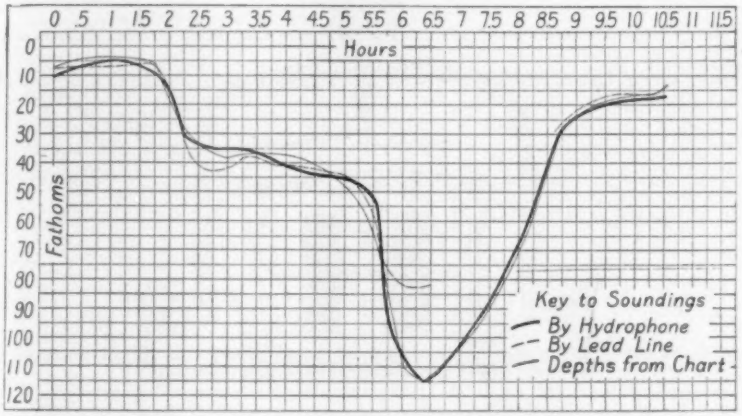
Comparative sounding data, electrical Hydrophone M V-62. *U. S. S. Breckenridge* en route from Charleston to Key West, January 14, 1920.

than a minute apart, while the vessel was not steaming over five knots, ofttimes showed a discrepancy of five or six fathoms. Under such conditions it was not expected that the acoustical sounding data would be at all accurate. However, the curves seem to show that the soundings given by the hydrophone are perhaps more reliable than any of the others. It is certain that these soundings, which are represented by the full heavy line, do not depart from the charted values, which are represented by the light full line as much as do the soundings taken by the lead-line or the sounding machine. Moreover, it will be noticed that, except at the 138-mile mark, the acoustical curve passes through

every point where two or more of the other curves coincide. This fact tended to make all who took part in the test believe the acoustical sounding data were, on the whole, most reliable.

The sounding data represented by the curves of Fig. 9 were taken during a run from Newport, Rhode Island, out to the hundred-fathom curve and back, and show the accuracy with which the apparatus can give soundings where the sea-bottom is somewhat regular. It will be noticed that the acoustical sounding data agree very closely with the charted depth except at the beginning and end of the run where the water was shoal. In these

FIG. 9.



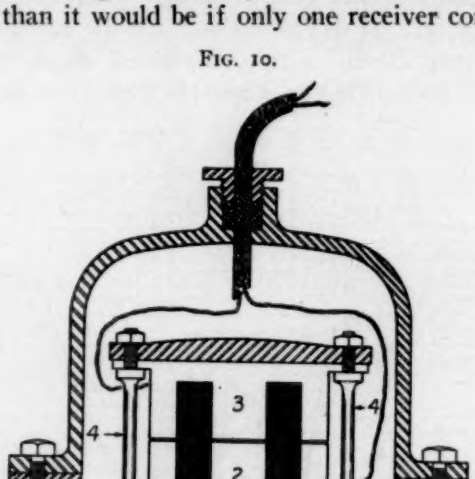
Comparative sounding data, acoustical Hydrophone M V-16B. *U. S. S. Blakeley*-150. Cruising off Montauk Point, May 27, 1920.

regions the agreement with the hand-line data is close. All who took part in this test believe the charted depths in the shoal region are too small for the reason that the hand-line soundings consistently gave about two fathoms greater depth; and there was a general feeling that the acoustical data represented the depth very accurately at all times.

The term " M V – Hydrophone," used in the caption of both Figs. 8 and 9, refers to the type of submarine sound receiver used for determining the time increment (ΔT). This type of receiver employs several sound receptors instead of two as described above. The receivers are equally spaced along a straight line passing through the propeller (P) and so connected through the compen-

sator that the forward half of the receivers connects with one ear in place of a single receiver as described, and the receivers of the rear half of the line connect through the compensator to the other ear. The compensator itself is so ingeniously designed that when any sound striking the receivers is binaurally centred the responses to this sound from all the receivers arrive at the ears in phase. This results in making the intensity of the received sound considerably greater than it would be if only one receiver connected with

FIG. 10.



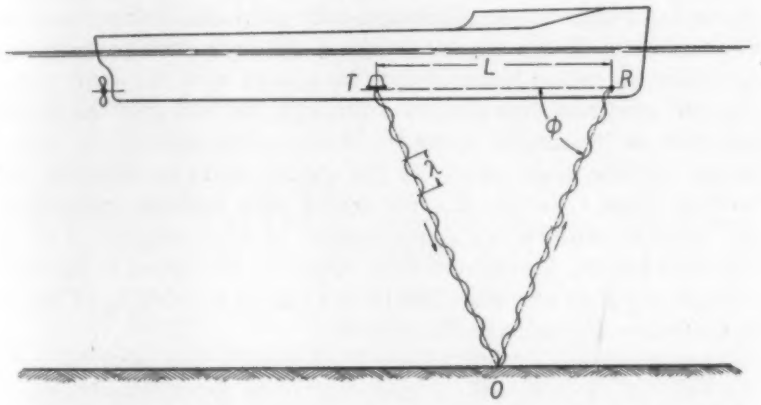
Schematic diagram of a submarine sound transmitter.

each ear. But this is not the only advantage of this type of receiver. Any sound that reaches the receivers and is not binaurally centred will have the responses from the several receivers arriving at the ears out of phase and they will partially destroy one another by destructive interference with the result that the sound heard is less intense than it would be if only one receiver connected with each ear. The M V – Hydrophone, therefore, can be focussed on any sound that the operator desires to hear, so that this particular sound gives a loud, clear response at the ears while all other sounds, and hence the local disturbing sounds, are greatly weakened.[3]

[3] A description of the M V – Hydrophone will be found in the *Marine Review* of October, 1921.

The ship's propellers form a convenient sound source for use in determining depths by the angle of reflection method, but any submarine sound transmitter of the oscillator type serves equally well. The principle of operation of such a sound transmitter can be understood by reference to Fig. 10, wherein numeral 1 indicates a rigid diaphragm in contact with the water, numeral 2 represents one-half of a powerful electromagnet rigidly attached to the diaphragm, and numeral 3 represents the other half of the electromagnet which is suspended in position by the elastic steel rods represented by numerals 4. When an alternating current is passed

FIG. 11.



through the magnetizing coil, the suspended half of the magnet vibrates back and forth alternately compressing and stretching the rods by which it is suspended and exerting a powerful thrust and pull on the heavy diaphragm that may equal several tons. Because of the incompressibility of water, it becomes necessary to exert great forces on the diaphragm to produce even a slight amplitude of motion. The submarine sound oscillator can be used for sending code or any other kind of signals by placing a key in the alternating current circuit.

The "Standing Wave Method" of determining depths can be understood by referring to Fig. 11 wherein T represents a submarine sound transmitter and R represents a submarine sound receiver, the two being separated a distance (L). The sound transmitter is driven by an alternating current supply so designed

that the frequency of the current can be controlled and varied by the operator over the range included between about 500 and 1500 cycles per second. This circuit is provided with means for giving the frequency with a high degree of accuracy at all times. The operator uses a two-telephone head set, one phone of which is inductively connected with the A. C. circuit that drives the sound transmitter. This connection is preferably made through a vario-coupler. The other phone is connected with the output from the sound receiver (R). With the phones connected in this way, it will be seen that the sound heard in the inductively connected phone has, at all times, a definite phase relation with the sound waves leaving the transmitter and the sound generated by the other phone has a definite phase relation with the sound waves reaching the receiver. If the operator adjusts the frequency properly, he can make the sound heard in the two phones have the same phase and can recognize this condition through the fact that the sound will then be binaurally centred. If the sound leaving the transmitter has the same phase as the sound waves arriving at the receiver, then ($T-O-R$), the sound path between transmitter and receiver, represents a whole number of wave-lengths. Calling this path-length, S; and the time required for sound to travel a distance equal to one wave-length, ΔT; and the velocity of sound in sea-water V, we have the relation:

$$(1)\quad S = V.N.\Delta t$$

where $N.\Delta t$ represents the time required for the sound to travel the whole path-length. But Δt, which represents the time for one wave to pass a fixed point, is equal to one second divided by the frequency (n) of the sound. Expressed as an equation, this becomes:

$$(2)\quad \Delta t = \frac{1}{n},$$

and by substituting this value in equation (1), we have:

$$(3)\quad S = N \cdot \frac{V}{n},$$

and the path-length (S) between transmitter and receiver would be known if the number of wave-lengths (N) in the standing wave system were known.

The value of N can be found by varying the frequency of the sound until a second standing-wave system is established.

Suppose the operator adjusts the frequency so that the sound is binaurally centred. Call the frequency (n_1). This will be equal to the frequency of the A. C. current. Then suppose he slowly raises or lowers the frequency. He will notice that the sound apparently passes away from binaural centre and finally comes back across centre. Each time the sound comes back across binaural centre, he has varied the number of waves in the standing wave system by one. Suppose he varies the frequency until the number of waves in the system differs from the number in the original system by a and he determines this number by counting the number of times the sound comes to a binaural centre while he slowly changes the frequency. Call the final frequency which he carefully adjusts for a binaural balance, n_2. Then since the path-length (S) is the same in the two cases, we have:

$$(4)\quad S = N\frac{V}{n_1} = (N \pm a)\frac{V}{n_2}$$

wherefore $(5)\quad N = \pm a\,\frac{n_1}{n_2 - n_1}$,

the sign before a being such as to make N positive. Substituting this value for N in equation (4) gives:

$$(6)\quad S = \frac{aV}{n_2 - n_1},$$

which gives a very simple working formula for determining S, the sound-path from the transmitter to the receiver by way of reflection from the sea-bottom.

It will be noticed that the determination of S does not furnish sufficient data for calculating the depth except when the sea-bottom is horizontal as shown in the figure. The point of reflection (o) may be anywhere on the surface of an ellipse having T and R as the two foci and the sum of the radius vectors $(O-T)$ and $(O-R)$ equal to S. If, however, the M V – Hydrophone is used for the receiver, the angle (ϕ) that the radius vector $(O-R)$ makes with the principle axis $(T-R)$ can be readily determined. This furnishes data for the complete solution of the depth and the slope of the sea-bottom.

This method has proved to be extremely accurate for measuring shallow depths. It gives greater accuracy as the depth becomes more shallow for the reason that the less the depth the greater the change of frequency that is required to change the number of

waves in the standing wave system. The method breaks down at great depths for the reason that a small change in the frequency introduces so many waves into the system and so rapidly that they cannot be accurately counted and thus the factor a becomes indefinite.

From the above descripion, it might be thought that each sounding taken by the standing wave method requires the solution of a problem in conic sections and would therefore be slow and cumbersome in practice. Such, however, is not the case. Having determined the value of S and ϕ, the depth can be determined from a family of curves with very little effort or loss of time.

It will be noticed that the apparatus employed for utilizing the standing wave method of determining depths is practically the same as that used with the angle of reflection method. The same receiver and transmitter serves equally well for both methods, but the standing wave method requires, in addition, some means for controlling the pitch of the transmitted signal and a frequency meter for determining the pitch.

The method, as outlined, has been found to give accurate results and to be easily applied. It can be modified somewhat without impairing its efficiency. Instead of energizing one phone by inductive connection with the A. C. circuit of the transmitter, it can as well be energized by a second receiver located near the transmitter. And instead of energizing the two phones separately so that the binaural sense can be utilized for adjusting the frequency to bring about a definite phase relation between the sound leaving the transmitter and that reaching the receiver, both currents can be passed through both phones in parallel, or series, in which case the observer can adjust the frequency to give a maximum or minimum response in the phones. He will then determine a by counting the number of maxima or minima, respectively, that occur during the change of frequency from the first to the second adjustment. This arrangement can be used by an operator who is deaf in one ear.

The " Echo Method " of determining depths is very similar to the standing wave method. It consists of sending out a continuous series of short sound signals separated by equal time intervals and means for varying continuously the time interval between successive signals from about one-tenth of a second to

about ten seconds, the means being such that the interval between successive signals can be accurately determined. If S represents the distance the sound travels in passing from the transmitter to the receiver by way of reflection from the sea-bottom, t represents the time of transit and V represents the velocity of sound in sea-water, then we have the relation:

$$(1)\quad S = V.t,$$

and it becomes necessary to determine t.

If the period between successive signals is made such that a signal returns to the receiver at the same instant that one is sent out from the transmitter, then we have the equivalent of a standing wave system, and if p represents the time interval between successive signals we have the relation:

$$(2)\quad t = N.p,$$

where N is the number of signals that are in transit between the transmitter and receiver. Substituting this value for t in equation (1) gives:

$$(3)\quad S = V.N.p.$$

The factor V is known and the factor p can be determined by the apparatus that is employed to automatically close and open the A. C. circuit through the transmitter. This apparatus, which serves as an automatic sending key, will be described later.

To determine N the operator will vary the frequency at which the sound signals are transmitted until the signals and echoes are heard simultaneously in the two telephone receivers, one of which is inductively connected with the A. C. transmitter circuit and the other of which is connected through the compensator of the M V – Hydrophone to the submarine sound receivers. This is the same arrangement that is employed in the standing wave method. The operator may continue varying the frequency of the signals until the coincidence between signals and echoes has arrived and passed by a times to the final adjustment. Then since the distance the sound has travelled between transmitter and receiver has remained constant, we have the two equations:

$$(4)\quad S = V.N.p_1 = V.(N \pm a).p_2$$

where p_1 and p_2 represent, respectively, the time interval between successive signals in the two cases. Solving for N, we find:

$$(5)\quad N = \pm a \frac{p_2}{p_1 - p_2}$$

and by substituting this value for N in equation (4), we have:

$$(6)\quad S = V.a. \frac{p_1 p_2}{p_1 - p_2}.$$

It is obvious that the sign of N must be such as to make S positive.

Equation (6) can be simplified by replacing the p factors, which represent the time between signals, by a set of n factors, representing respectively the frequency of the signals. If this is done, equation (6) reduces to:

$$(7)\quad S = \frac{V.a}{n_2 - n_1},$$

which is identical with the sounding equation developed in connection with the standing wave method.

The automatic transmitting key, that must serve for varying the value of p and for determining its value, might be a pendulum with arrangements for varying its period of vibration and for closing and opening the transmitter circuit. If this were done the factor p or its reciprocal $\frac{1}{n}$ would be expressed in terms of the well-known pendulum laws. But since pendulums do not work well on board ships, it is preferred to use a disc caused to revolve at constant speed, similar to the revolving plate of a graphophone, and a small friction wheel resting on this surface with means for moving it inwards or outwards along a radius of the constant speed disc. With this arrangement the speed of the friction wheel can be varied continuously from zero to a value that is dependent upon the speed of the disc and the ratio of the radius of the disc to that of the friction wheel. The closing and opening of the transmitter circuit is accomplished by contact points operated by a cam-wheel attached to the axis of the friction wheel. If this cam carries one tooth, one signal will be transmitted for each revolution of the friction wheel, while if the cam carries C teeth equally spaced about its circumference there will be C signals transmitted for every revolution of the friction wheel.

Suppose the constant period of revolution of the disc is P seconds and that the radius of the friction wheel is r. Also sup-

pose the distance from the centre of the disc to the point of contact between the disc and friction wheel is R. Then since there is no slip between the friction surfaces we have:

$$(8)\quad \frac{2\pi R}{P} = \frac{2\pi r}{p}$$

and

$$(9)\quad p = \frac{P \cdot r}{R},$$

if the cam-wheel carries one tooth, for then the period p between signals is the same as the period of revolution of the friction wheel. If, however, the cam-wheel carries C uniformly spaced teeth, then:

$$(10)\quad p = \frac{P \cdot r}{C \cdot R},$$

and

$$(11)\quad n = \frac{1}{p} = \frac{C \cdot R}{P \cdot r}.$$

Substituting this value in equation (7) gives for the sounding equation:

$$(12)\quad S = \frac{a \cdot V \cdot P}{C(R_2 - R_1)},$$

where a is the number of signals in transit that have been introduced in passing from the first to the second condition for coincidence between signals and echoes, V is the velocity of sound in sea-water, P is the constant period of revolution of the disc, r is the radius of the friction wheel, C is the number of teeth on the cam that operates the contact points for closing and opening the transmitter circuit, R_1 is the distance from the centre of the disc to the point of contact with the friction wheel for the first adjustment for coincidence between signals and echoes in the two phones and R_2 is the corresponding distance for the second similar adjustment.

The accuracy with which depths can be determined by this method depends upon the degree to which the speed of the disc can be kept constant and the accuracy with which R can be measured. In practice the disc is driven by a tuning-fork speed-controlled motor operating through a worm and gear. With this arrangement the speed of the disc is kept constant to within a tenth of a per cent. The friction wheel is moved out and in across the

disc by means of a spiral thread operating in a nut, and the position of this wheel on the disc is determined by means of a micrometer scale on the end of the member carrying the spiral thread. This arrangement permits of measuring R to within .002 inch.

A factor that lends for accuracy in adjusting the friction wheel results from the fact that each signal echoes back and forth between the sea-bottom and surface several times. And, if the interval between signals is accurately adjusted for coincidence with any echo, the multiple echoes will all be coincident; while if the adjustment is not quite accurate, the multiple echoes will vary from coincidence two, three, or n times as much as does the first echo. This dispersive effect of the multiple echoes proves to be an aid in making an accurate adjustment for determining depth, whereas they prove to be a disturbing factor in case of Fessenden's method that has been described. It has been demonstrated in practice that the value of R can be determined to within two or three-thousandths of an inch and this results in measuring the time of sound transit to the sea-bottom and back to within one five-hundredth of a second.

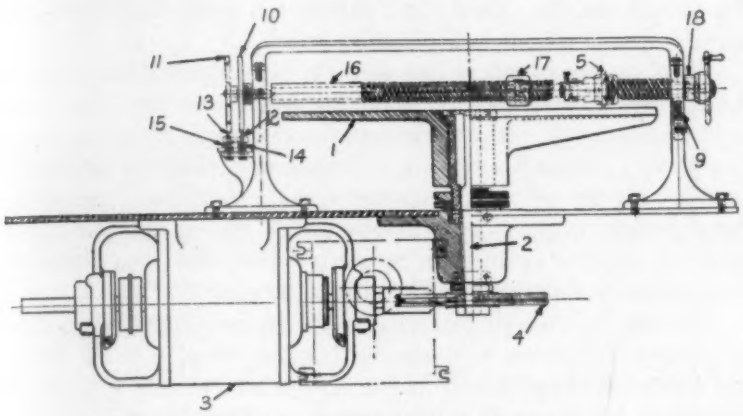
The velocity of a sound in sea-water for moderate depths and average temperature is about 4800 feet per second. But the velocity (V), which can be accurately expressed as:

$$V = \frac{1}{\sqrt{\mu \cdot \rho}},$$

where μ is the adiabatic compressibility and ρ is the density, is evidently affected by both the temperature and pressure of the water and will, therefore, vary with the depth. The variation of the temperature of sea-water with the depth is not well known over most of the ocean areas, but it is well established that the temperature conditions below 1000 fathoms or even 500 fathoms do not vary greatly. Therefore, when the velocity is determined for depths beyond these values, the variation of depth, which alone is of value in determining submarine contours, can be determined to within an error represented by the distance that sound travels in sea-water in about one five-hundredth of a second or to within about ten feet. But since in determining depths the distance S that the sound travels in going from transmitter to receiver is divided by two, the error is also halved, and the depth variation will be determined to within about a fathom.

The variation in the velocity of sound, as it proceeds to greater depths, that is produced by increase in pressure, is in opposition to the variation produced by the change of temperature when, as usual, the temperature decreases with increasing depth. As a result the average value of 4800 feet per second for the velocity of sound gives sounding data accurate to within 1, or at most, 2 per cent. The determination of the velocity of sound in seawater under various conditions of temperature, pressure and salinity is being undertaken by the Navy and its solution will

FIG. 12.



Practical design of sonic depth finder.

increase the absolute accuracy with which soundings can be taken by the echo method.

Fig. 12 shows the nature of a practical design of the automatic signalling key as developed at the Engineering Experiment Station, U. S. N., Annapolis, Maryland. A disc (1) twenty inches in diameter, carrying a vertical axis (2), is driven at a constant speed of one revolution in ten seconds by means of a dynamotor (3) whose armature shaft carries a worm that engages in gear 4 which is keyed to axis 2 at its lower end. The speed of the dynamotor is kept constant to within a tenth of a per cent. by means of a tuning-fork control for varying the load.

The top of disc 1 is covered with canvas or other suitable material for forming a friction surface for driving friction-wheel 5. This wheel, which is two inches in diameter, engages with shaft 6 by means of a slot and spline arrangement such that

member 5 can slide along member 6, but which causes both of these members to rotate in unison. The axis of shaft 6 is so mounted that it is parallel with the friction surface of the disc and intersects the axis of pinion 2 extended upward.

Shaft 6 is supported by self-aligning ball-bearings (7 and 8), the former of which is set in a groove such that it can slide a short distance up and down and thereby allow the pressure between members 1 and 5 to be adjusted by varying the tension on spring 9. It carries two cam-shaped discs near the end supported by bearing 8, one of which (10) carries a single saw-tooth-shaped depression and the other (11) carries ten such depressions uniformly spaced about its circumference.

Two spring members, 12 and 13, bear against the two cam discs, respectively, and by snapping down into the depressions as the cams rotate, serve to operate the two pairs of contact points (14 and 15) in such a way as to temporarily close the alternating current circuit of a submarine sound oscillator transmitter. Arrangement is provided whereby either spring member can be made to operate against its respective cam, but which prevents both members from operating at the same time.

Member 16, which carries a spiral thread engaging in a nut in member 17, serves to move the friction wheel 5 along shaft 6 and measures the radius R of the circle that member 5 scribes on member 1 by means of a micrometer scale on cylinder 18. The smallest scale division represents one one-hundredth of an inch and readings can be estimated to tenths of a division.

The sounding equation,

$$S = 2D = \frac{a \,.\, V \,.\, P \,.\, r}{C\,(R_2 - R_1)},$$

when applied to this design of key has the following values for the several constants:

$V = 4800$ feet per second.
$P = 10$ seconds.
$r = 1$ inch.
$C = 1$ or 10, depending upon which cam is used.

Inserting these values, we have for D the depth:

$$D = \frac{48000 \,.\, a}{2\,C\,(R_2 - R_1)} \text{ feet}$$

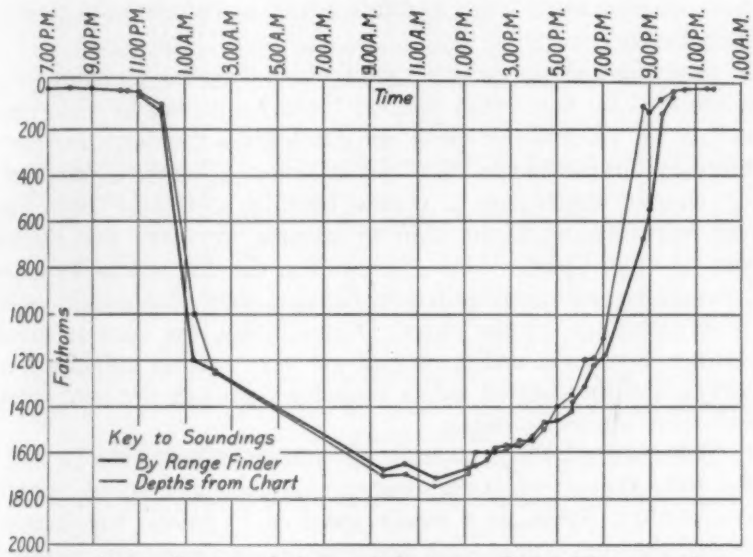
or

$$D = \frac{4000 \,.\, a}{C\,(R_2 - R_1)} \text{ fathoms,}$$

where, as before stated, a is the change in the number of signals in transit, brought about by varying the adjustment of the friction wheel through the radial distance $(R_2 - R_1)$, and C is one or ten, depending on which cam-wheel is employed.

When both C and $(R_2 - R_1)$ are given their greatest value, 10 in each case, it will be noticed that the device is then adjusted for measuring its most shallow depth, which is 40 fathoms. With this adjustment the time interval between successive signals is

FIG. 13.



Comparative sounding data. Sonic range finder, type 2. *U. S. S. Ohio* en route from New York to Annapolis, February 13-15.

one-tenth of a second and the value of a is one. Under such conditions each signal echoes back to the receiver at the instant the next successive signal is transmitted and the determination will be very accurate for the reason that the value $(R_2 - R_1)$ will be large (ten inches) and any small error in determining this value will only introduce a small percentage error. But suppose a depth of 4000 fathoms were being measured. Then in order that an echo should reflect to the receiver at the same time the next successive signal is transmitted the time interval between successive signals would need to be ten seconds and the value of $(R_2 - R_1)$ would be only one inch. Under such conditions the

slight error made in measuring $(R_2 - R_1)$ would result in ten times as much percentage error as in case of the shallow measurement. If, however, we send the signals ten times as fast, *i.e.*, once per second, then there would be ten echoes in transit and a would become ten and the value of $(R_2 - R_1)$ would now become ten inches, the same as in case of the shallow depth measurement, with the same resulting error in our 4000-fathom sounding that was made in determining the shallow sounding. By choosing a proper value for a, the value of the measured factor $(R_2 - R_1)$ can always be made large, and this results in reducing the experimental error.

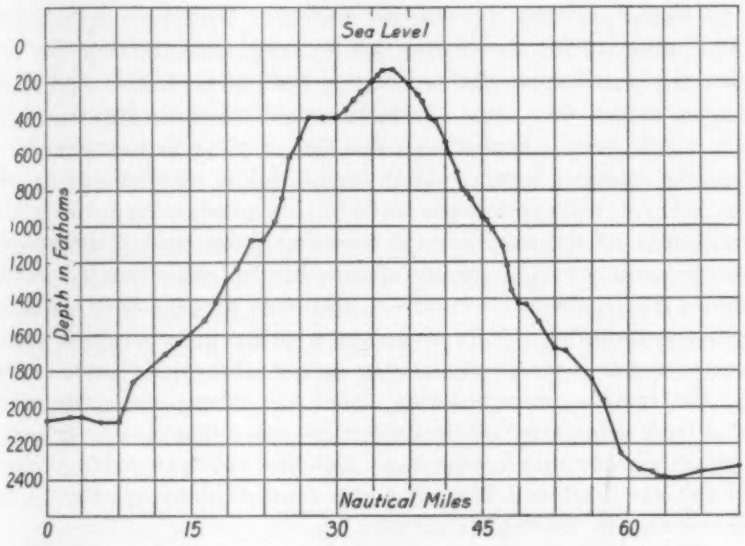
Some idea of the accuracy with which deep-sea soundings are determined by the "echo method" can be gained by referring to Fig. 13, wherein the heavy line curve gives the depth as determined by this method and the light line curve gives the corresponding charted depth over a course bearing southwest from the Ambrose Channel Light Ship to latitude 37 north and thence west to Cape Charles Light Ship. The charted depths in some instances represent interpellations between somewhat widely separated soundings on the chart. Nevertheless, the data included between 2:00 A.M. and 6:00 P.M. are over a fairly uniform sea-bottom and the charted values probably represent the true depth with considerable accuracy.

A line of soundings taken by the author on board the U. S. S. destroyer *Stewart* while she steamed from Newport, Rhode Island, to Gibraltar, Spain, at a steady speed of 15 knots, has demonstrated the ruggedness, ease and rapidity of operation, accuracy and dependability of the acoustical depth-sounding apparatus that has been described. The apparatus at no time failed to function properly and required no repairs or adjustments during the entire trip. The average time required to make a sounding was about one minute. Throughout the course soundings were taken at least every twenty minutes, and in regions where the depth varied somewhat rapidly they were taken at times as often as every minute.

These soundings determine the profile across the Atlantic along a great circle route from Newport to the Azores and from thence across the Josephine and Gettysburg Banks to Gibraltar. This profile cannot be reproduced in its entirety, but some idea of

the accuracy with which it represents the variations in depth can be gained by considering Fig. 14, which refers to the Gettysburg Bank along a section defined by the course of the *Stewart*. At the time this Bank was discovered by the *U. S. S. Gettysburg*, soundings showed the bottom to consist of sand and rounded pebbles. The profile of Fig. 14 gives evidence of old shore

FIG. 14.



Contour of the Gorringe or Gettysburg Bank. Soundings taken by *U. S. S. Stewar* June 28, 1922.

terraces at a depth of 400 fathoms, and it therefore appears probable that this region has been above sea-level at some remote age.

During the months of October and November, 1922, the U. S. S. destroyers *Hull* and *Corey*, using the echo method, made a survey of the ocean floor along the California coast from San Francisco to Pt. Descanso from the hundred-fathom curve out to a depth of 2000 fathoms. The area covered was approximately 35,000 square miles and the work was accomplished in thirty-eight days. Over 5000 soundings were taken while the ships steamed steadily at twelve knots. There is no doubt but that these soundings, as assembled in chart form by the Hydrographic Bureau of the U. S. Navy, represent the contour of the sea-bottom with considerable accuracy even though the survey

was made at the rate of about 1000 square miles per day. This survey has demonstrated beyond a doubt that the ocean beds can now be charted with a high degree of accuracy and that the survey work can be done with a speed and an economy of expense and effort that has heretofore been believed impossible.

The data represented by this chart furnish subject-matter for an extended paper and, therefore, cannot be covered at this time. It may be stated, however, that this region has been surveyed for the reason that seismographic records for the past ten years show that numerous earthquakes have had their origin within this area. It is proposed to re-survey this area after such records have shown that one or more earthquakes have had their origin herein. It is hoped that a comparison of the two charts will give some definite information regarding the movement of the earth's crust resulting from such disturbances. A comparison of the present chart with the older charted values shows discrepancies of hundreds and even thousands of fathoms in some localities. It is uncertain whether these marked variations are due to errors made in the earlier surveys or to movements of the earth's crust resulting from the numerous earthquakes that have originated within this region since that time. It seems certain that repeated surveys of this and other unstable regions of the sea floor will furnish much valuable data relating to the movement of the earth's surface.

The application of the methods and apparatus for taking depth soundings that have been described are more numerous and valuable than one might at first suppose, as has become evident to the author through the many letters of inquiry that he has received.

Their value as an aid and safeguard to navigation has been repeatedly proved on various vessels of the U. S. Navy. And their value does not cease when a vessel steams into ocean depths for such survey work as has already been done shows that the deep-sea mountains and valleys will furnish numerous "landmarks" for determining the progress of a vessel, as soon as the main trade routes have been carefully charted. Moreover, the M V – Hydrophone receiver, besides determining the direction of sound signals used for sounding purposes, will equally well determine the direction of such signals transmitted from other moving vessels or

from light vessels placed at harbor entrances or at dangerous points along shore. In this way it serves to prevent collisions during conditions of low-visibility and to direct vessels safely into harbor or away from dangerous rocks and shoals. If all ships were equipped with the sound apparatus that has been described and would sound their submarine sound transmitter during fog, the navigator could then know the bearing of every vessel within a radius of at least ten miles, in addition to knowing the depth of water underneath his own vessel. With such information at his disposal, the grounding of, or collision between, vessels could be absolutely avoided.

These sound devices will serve to make a cheap, quick and accurate survey of rivers and harbor entrances through which a channel is to be dredged, thereby furnishing accurate data for computing the amount of material that must be moved. They will also serve to determine the capacity of reservoirs with a minimum of effort and expense.

A study, during the flood period, of the beds of such rivers as the Yangste, the Mississippi and others that are wont to overflow and cause great loss of both lives and property, would doubtless furnish much valuable information for controlling such streams. The velocity of the water is usually so great that soundings cannot be made with the hand-lead, but by means of the "standing wave method" the beds of such rivers can be surveyed with great accuracy. Such surveys made at numerous sections of the stream should show over what portion erosion takes place and what is more important, should show where the sediment is being deposited. If erosion at the bottom of the stream becomes active when the velocity of flow exceeds a certain minimum value (as is believed by some engineers), and if it can be determined what this minimum velocity is, then it is quite possible that the proper method of controlling the stream will be to narrow its confines rather than to widen them or raise the dykes, for by so doing the required cross-section to carry the flow will be gained by deepening the stream through the process of erosion. The possibilities of the depth-sounding devices for use in this way are perhaps far greater than we can now appreciate.

The "echo method" of determining depths is not confined to determining submarine depths. It should serve equally well for

determining the depth below the earth's surface of abrupt changes or discontinuities in the earth's crust such as are offered by coal and oil deposits or subterranean caverns. These surfaces of discontinuity will reflect to the surface a part of any sound disturbance that may be transmitted to them. And though it may seem far-fetched, there is a possibility that the methods outlined may also be utilized for locating cracks and blow-holes in large castings.

The apparatus employed with the "echo method," which has been described as a means for determining the distance between two points in a uniform medium when the velocity of sound is known, serves equally well for determining the velocity of sound between two points in any medium when the distance they are separated is known. In this connection this apparatus may serve to determine the velocity of sound through the rock formation of a mountain or between borings or workings in mining operations. And since the velocity so determined is equal to the square root of the elasticity of the formation divided by its density, this information may lead to the identification of valuable ore.

Of the above-named applications of the acoustical depth-finding devices that have been described, only two have been put to practical test. Their ability to aid and safeguard navigation during conditions of low visibility has been repeatedly demonstrated on ships of the Navy, and the survey of the sea floor off the California coast together with a more recent survey of a region off the entrance to the Panama Canal has proved that the sea floors can now be accurately and easily mapped. An idea of the value of these devices resulting from these two applications alone can perhaps be best conveyed by quoting from a prominent captain of the U. S. Navy, who states, "If it became necessary to dispense with the gyro compass or acoustical depth-finder on my ship, I should much prefer to keep the depth-finder apparatus," and finally from one of our well-known geologists who says, "Now for the first time in history the practicability of securing a map of the vast unknown area of the ocean floors is assured, with a promise of such a wealth of scientific data of prime importance as to make these developments of the U. S. Navy rank very high among those which have advanced our knowledge of the earth."

A SINGULAR CASE OF ELECTRON TUBE OSCILLATIONS.*

BY

G. BREIT.

National Research Fellowship, University of Minnesota.

WHILE trying to reproduce the oscillations described by Barkhausen and Kurz [1] with a three-electrode tube of an experimental type, a singular oscillation of an apparently new type has been observed. The arrangement of circuits is that used by Barkhausen and Kurz. The plate is disconnected and the grid is connected to the positive pole of a 240-volt battery through a protective resistance of say 500 or 2000 Ω. High-frequency oscillations of a wave-length of about 60 or 50 centimetres result if the filament current has the right value. If the filament current is below or above that critical value the oscillations stop. The range of variation of the filament current for which the oscillation occurs is not very regular, but is generally of the order of 0.05 ampere. The writer has no satisfactory explanation of the phenomenon and for this reason the general facts observed are recorded below without discrimination.

The tube is of an experimental General Electric type. Several of these have remained at the Cruft Laboratory since the war. The first observations have been made at the Cruft Laboratory. Through the courtesy of Professor Chaffee, three tubes have been loaned to the writer for use at the University of Minnesota. The filament and grid of the tube are helical. The filament is of tungsten. The grid is very close to the filament. The diameter of the grid is about 3 millimetres. The plate is cylindrical and coaxial with the grid and filament. Its diameter is about 1.2 centimetres and its height is roughly the same as its diameter. The lead to the plate is brought in through a separate seal at the top of the tube. The tube is apparently a hard, high vacuum tube. It may be used with 500-volt plate voltage in spite of the heating of the electrodes.

The oscillation was first observed at the Cruft Laboratory

* Communicated by Dr. Joseph S. Ames, Associate Editor of this JOURNAL.

[1] *Phys. Z. S.*, **21**, p. 1, 1920.

with a tube which is referred to below as 1. Approximate measurements of the wave-length were made by observing the resonance of two pieces of telescoping copper tubing. Multiplying the resonant length by 2, values in the neighborhood of 60 centimetres resulted. In these experiments a by-pass condenser was connected across the high-voltage battery and the plate was connected to the filament. At times oscillations were observed which had apparently a longer wave-length. Their relation to the first appears clearer below.

When tube 1 was tried in Minneapolis with apparently the same connections, the oscillation did not appear the first day. It appeared weakly the second day when the grid was disconnected from the by-pass condenser. However, measurements with the resonator gave no tuning, the wave-length being apparently higher than 60 centimetres. Lecher wire measurements showed the wave-length to be about 150 centimetres and the filament current at which they occurred was lower ($I_F = 1.45$ amperes) than that used to get oscillations in Cambridge. On disconnecting the by-pass condenser entirely, this oscillation disappeared, but at $I_F = 1.60$ amperes a short wave-length oscillation appeared which, according to the Lecher wire measurements, corresponds to 60 centimetres wave-length. Both of these depend critically on I_F and with the by-pass condenser disconnected, traces of the $I_F = 1.45$ are at times seen. The plate was next disconnected and the intensity of the oscillations was increased thereby very markedly, while the wave-length was shortened to about 48 centimetres or 50 centimetres.

The protecting resistance in the grid circuit was varied so as to give different grid currents (30 to 60 m. amperes). This affected the amplitude of the oscillations, but apparently their frequency was not affected more than it was liable to change from time to time in repeating measurements with the same protecting resistance. Measurements made with different grid currents in immediate succession gave no change in the wave-length.

It was thought that the effect may be due to a sagging of the filament owing to its high temperature and to a consequent tuning of the small helix of the filament to that of the grid or to some other electrical circuit. This, however, is apparently not the case, because if the tube is put on its side the oscillations are not disturbed.

It was thought that the filament current or the filament potential are instrumental in bringing about the oscillation in virtue of some peculiar synchronizing action on the electrons. At a certain stage this possibility seemed feasible because under the action of the filament potential the time taken by an electron to traverse the length of the filament is of the order of magnitude of the period of the oscillation observed. This, however, was also shown not to be connected with the cause of the oscillation; an alternating current heating the filament gave very nearly the same results as the direct current as long as its *effective* heating value was the same. Thus it is the amount of heat supplied to the filament by the filament current that matters and not the filament current itself.

This is further supported by observations on the change in the value of the critical filament current with the grid current. The electrons leaving the filament and going to the grid are not effective in heating the filament. The changes observed are in the right direction and of the right order of magnitude, though they were not observed accurately enough to be considered as an independent, quantitative confirmation.

Tube 2 appears to be identical with tube 1. Tube 3 has a burned-out seal which probably resulted in introducing a slight amount of gas into it. Its behavior is noticeably different from that of 1 and 2. The oscillation occurs at a lower filament current: $I_F = 1.45$ amperes $\rightarrow 1.50$ amperes as compared with $I_F = 1.60$ amperes $\rightarrow 1.65$ amperes for 1 and 2. The adjustment for the oscillation is easier for 3. The range of I_F is slightly wider. The intensity of the oscillation is likely to be greater. It is more reliable as an oscillator; 1 and 2 oscillate with difficulty if they have not been used for a week. Thus the presence of gas to a slight degree is beneficial.

The power dissipated by 3 in its filament at the critical temperature is less than that of 1 and 2. The resistance of the filament of 3 is nearly the same as that of the two other tubes. Thus its temperature is likely to be different. A visual comparison of the brightness of the filaments of 1 and 3, made by means of absorbing cells of potassium dichromate or potassium permanganate, indicates this too. Thus we have evidence that it is not the temperature directly that matters. One is tempted to suppose

that a secondary effect of temperature—such as the pressure—brings about the oscillation.

It must also be stated that the oscillation shows hysteresis. If the plate is touched with one's finger, the oscillations drop and go on dropping after the finger is removed. If at this stage the filament current is increased beyond the oscillation point and then decreased again, a strong oscillation results.

Precautions have been taken to test for a possible Barkhausen-Kurz oscillation masked by the tuning effect of the filament current to the receiving apparatus. The latter consisted of a crystal detector and wall galvanometer. The leads to the galvanometer were twisted and a wire of roughly 10 centimetres length was connected to one of the binding posts of the crystal detector. Changing the wire, moving it about or using it as a loop by connecting to both crystal detector terminals made no difference in determining whether the tube was oscillating or not. A vacuum thermocouple was also used with the same galvanometer, and wave-length measurements made by the thermocouple agreed with those made by the crystal, thus showing that electro-elastic vibrations in the crystal have nothing to do with the phenomenon.

(The Lecher wire wave-length measurements were ordinarily made by means of the crystal. The Lecher wire system was coupled to the stationary crystal by means of a loop and a bridge was moved over the wires. The waves were reflected from the bridge and depending on their phase enhanced or diminished the direct influence of the oscillator on the detector.)

Basic Cupric Sulphate.—F. S. WILLIAMSON, of Cornell University (*J. Phys. Chem.*, 1923, **27**, 789–797), has studied the action of sodium hydroxide upon cupric sulphate in aqueous solution. When less than 1.5 molecules of the hydroxide react with one molecule of the sulphate, a light green precipitate is formed, which apparently has the composition $CuSO_4.3CuO.4H_2O$ or $CuSO_4.3Cu(OH)_2.H_2O$. It may possibly be a basic salt containing adsorbed cupric sulphate. When two molecules of the hydroxide react with one molecule of the sulphate, a brown precipitate of hydrous cupric oxide results. When 1.5 molecules of the hydroxide react with one molecule of the sulphate, a gray-green precipitate forms, which is presumably a mixture of the light green and brown compounds. J. S. H.

A PRIMARY STANDARD OF LIGHT, FOLLOWING THE PROPOSAL OF WAIDNER AND BURGESS.*

BY

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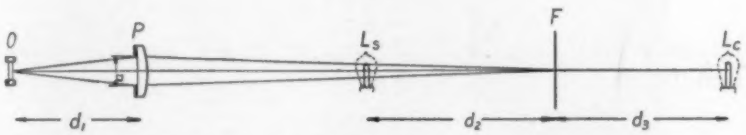
III. MEASUREMENTS BY A VISUAL METHOD OF THE BRIGHTNESS OF THE PLATINUM BLACK BODY AT ITS MELTING POINT.

(*a*) *General Method of Observation.*—As already stated, in the preliminary work a small diaphragm was placed immediately in front of the aperture of the platinum black body and the light coming from this aperture was allowed to fall on the photometer field. In the work here reported an entirely different method of observation was used. The method consisted in projecting an image of the black body opening upon one-half of the photometer field and measuring the illumination thus produced. The optical principle employed is that the brightness of the projecting lens as viewed from the plane of the image is equal to the brightness of

FIG. 22.



Scheme of optical system employed in visual observations.

the object, making, of course, the appropriate reduction for the absorption of the lens. This law is strictly applicable only to infinitely thin lenses. For the practical case of a thick lens the proper formulas may be deduced as follows:

In Fig. 22 let O represent the object (in this case the platinum black body); P, the projection lens; F, the photometer field; L_S, a standard lamp; L_C, a comparison lamp; d_1, d_2, and d_3, distances, respectively, between object and lens diaphragm, standard lamp and photometer field, photometer field and comparison lamp. Let b be the brightness in candle-power per square centimetre of the light source. Let a be the area of the lens diaphragm (placed on the side of the lens nearest the object) in square centimetres, and t the transmission of the lens. The luminous flux reaching the

* Concluded from the February issue, p. 182.

lens through the diaphragm, per square centimetre of the source, is equal to

$$\frac{ba}{d_1^2}.$$

Now this same luminous flux per square centimetre of the source, diminished by the absorption of the lens, reaches the photometer field; that is, the luminous flux, reaching the photometer field for each square centimetre of the source, is

$$\frac{bat}{d_1^2}.$$

Now if M is the linear magnification of the image on the photometer field as compared with the source, we see at once that the luminous flux reaching each square centimetre of the photometer screen per square centimetre of the source, is

$$\frac{bat}{M^2 d_1^2}.$$

Suppose now that the comparison lamp L_C is so set that there is a photometric match; let its candle-power be designated by C, let its distance be d_3, we then have that the luminous flux per square centimetre on the photometer screen is

$$\frac{C}{d_3^2}$$

and this must be equal to the luminous flux per square centimetre from the lens. Equating these two expressions

$$b = \frac{CM^2 d_1^2}{atd_3^2}$$

C may now be evaluated in terms of the standard lamp L_S, whose candle-power we designate by S, by placing the standard lamp at some convenient position, d_2, between P and F, and making a new setting of the comparison lamp. If the new position of the comparison lamp is d_4, we have C equal to

$$S\frac{d_4^2}{d_2^2}$$

which may be inserted in our value for b. In order to carry through our experiment it is necessary to determine a, t and M with high accuracy and to insure that the distance d_1 is accurately known and maintained during the experiments. In the apparatus as set up, a lens of approximately forty centimetres focal length and four centimetres diameter was used. This was a simple achromatic lens specially selected for freedom from specks, bubbles, and color. It was mounted on a photomicrograph object platform, which gave slow motion in altitude and azimuth (Fig.

23). The diaphragm *a* was made of paper-thin sheet metal pierced with a circular hole whose mean diameter was accurately measured on a dividing engine as 11.984 millimetres. Its area was, therefore, 1.1274 square centimetres. The distance between the slit of the platinum black body and the diaphragm was accu-

FIG. 23.

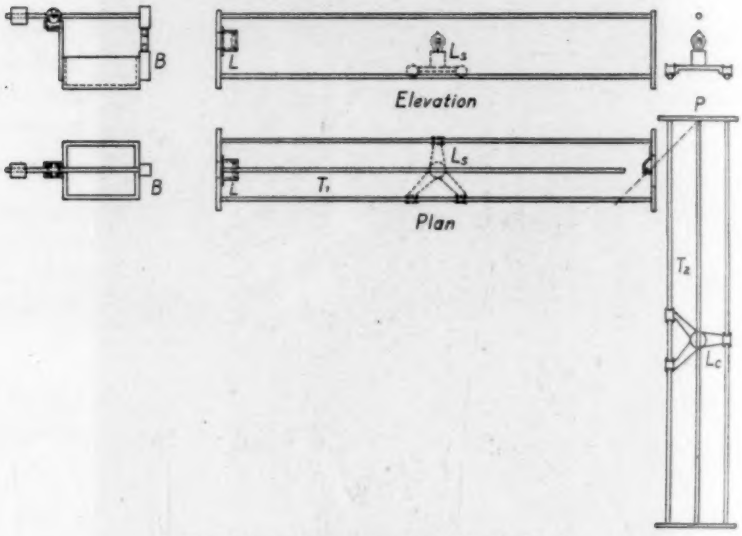
Lens mount.

rately 45.25 centimetres, and this was maintained throughout the work by occasionally referring to a length gauge, one end of which was inserted in the lens holder. The distance between the diaphragm and the photometer field was 228.55 centimetres. This latter distance was fixed by the construction of the photometer track, and the distance from platinum cylinder to diaphragm was fixed by very careful trial, so that the image was accurately focussed in the plane of the photometer field as determined by the parallax method of observation. As long as the

area of uniformly bright image is large, small errors of focus are without effect on the result. The highest accuracy is, however, necessary in the determination of the magnification.

In order to determine the magnification two methods were followed. The first consisted of measuring the size of the image formed at *F*, of a grating which was placed at *O*. This was measured by two methods: First, by visual observation with a

FIG. 24.



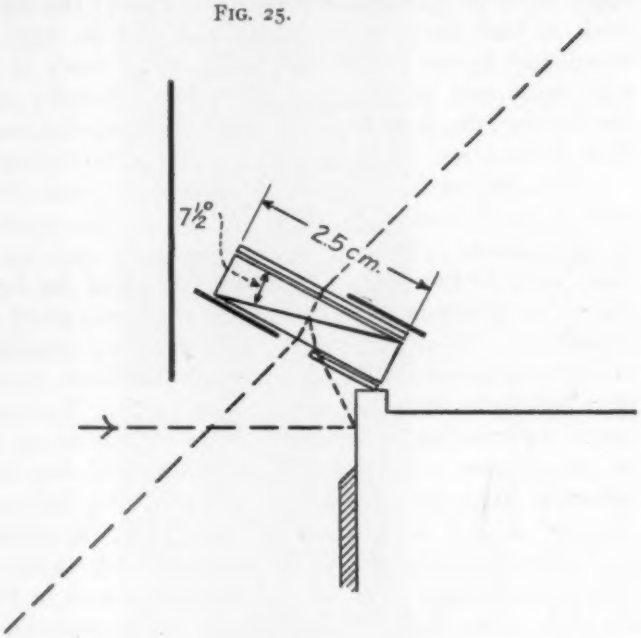
Plan and elevation of special photometer track.

micrometer microscope; and second, by means of photographs which were measured on the same dividing engine as the original grating. The mean of several measurements, both visual and photographic, which were in excellent agreement, gave for the value of *M*, 4.964. The second measure of linear magnification consisted in using the measurements of distance between the various parts of the system. Bearing in mind that the lens used was plano-convex, with its flat side toward the object, and its thickness .83 centimetre, we have for the distance of the front nodal point from the image 228.55 – .82 centimetre = 227.72 centimetres. The back nodal point is 45.25 +2/3 of .83 centimetre = 45.80 centimetres from the object. The ratio of these two figures gives, for the magnification, 4.972, agreeing with the first method

to better than one part in 500. The value used in the work was the mean of these two, namely, 4.968.

The transmission of the lens was measured on the right angle photometer track, which will be described later, by placing it between the two illuminated surfaces which constitute the elements of the photometer field, in which position it merely reduces the

FIG. 25.



Section through special contrast field parts.

amount of light reaching the eye from one field without affecting the illumination of that part of the field. The mean of five measurements, each involving twenty settings, made at various times during the progress of the work, gave a value for the transmission of .8878, which should be accurate to a tenth of 1 per cent.

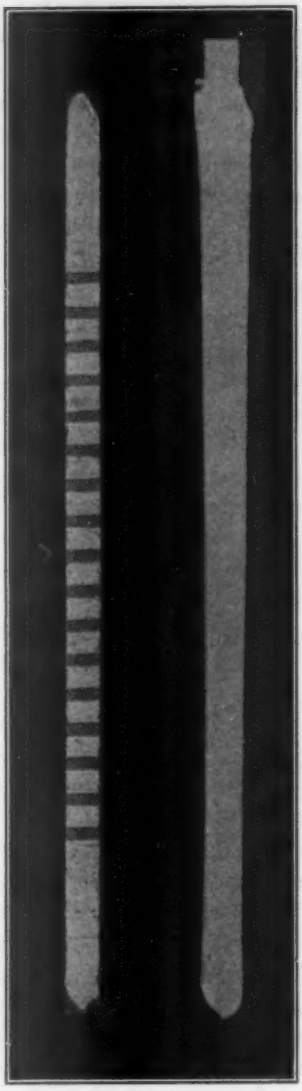
(*b*) *Photometric Arrangements.*—The photometer track and photometer field employed were built especially for this investigation. The track is shown in plan and elevation in Fig. 24. It consists actually of two tracks, T_1 and T_2, placed at right angles

to each other, so arranged that a large flat plate of smooth plaster-of-Paris, *P*, which is illuminated by the comparison lamp, is viewed past the edge of an opening at the end of the standard lamp track. At the other end of the track is the projection lens *L*, and beyond it the platinum black body *B*. It was the original intention to use a simple equality field made by placing a sharp-edged block of plaster-of-Paris at the end of the standard lamp track, so that the observer would look past its edge at the field illuminated by the comparison lamp. This form of simple field was abandoned in favor of a biprism, whereby the dividing line between the two plaster-of-Paris surfaces was made sharper. This, in turn, was finally given up for a special contrast field.

This contrast field was constructed in a manner which will be clear from the section shown in Fig. 25. In this figure the image of the platinum cylinder opening is projected upon the plaster-of-Paris strip at the lower right-hand corner of the figure, but is viewed by reflection from a silver on glass grid which is mounted between two 7½-degree prisms. The field thus presented consists of alternate strips of clear and reflecting surfaces, the comparison lamp field being seen through the clear spaces. To transform this into a contrast field a series of strips of microscope cover glass are placed over every fourth transmission and over every fourth reflection strip; the microscope cover strips on the inner side are so short as only to intercept the light from the plaster-of-Paris surface before reflection, those on the far side extend completely across the grating. The field produced is shown in Fig. 26. To the right is the image of the opening in the platinum cylinder as projected. To the left is the contrast field. The position of match is presented when the contrasting strips stand out with equal distinctness against the background formed by the equal brightness of the adjacent reflecting and transmitting strips. In order to indicate clearly which strips should be dark, in the absence of any distinctive difference of shape in the various elements, a small arrow head is cut in the diaphragm over the double prism element, as indicated to the left of the field in Fig. 26.

This contrast field was found to be highly satisfactory, giving a precision of setting which compares favorably with the regular Lummer-Brodhun type. The small difference in color introduced by the reflection from the silvered mirror was compensated for by lowering the voltage of the comparison lamp. The possibility of observing the direct image of the slit at the side of the comparison

FIG. 26.



Appearance of special contrast photometer field. To right, direct image of black body; to left, reflected image alternated with comparison field.

field at any time during the progress of the heating process insures that no accidental deformation or displacement will invalidate the measurements.

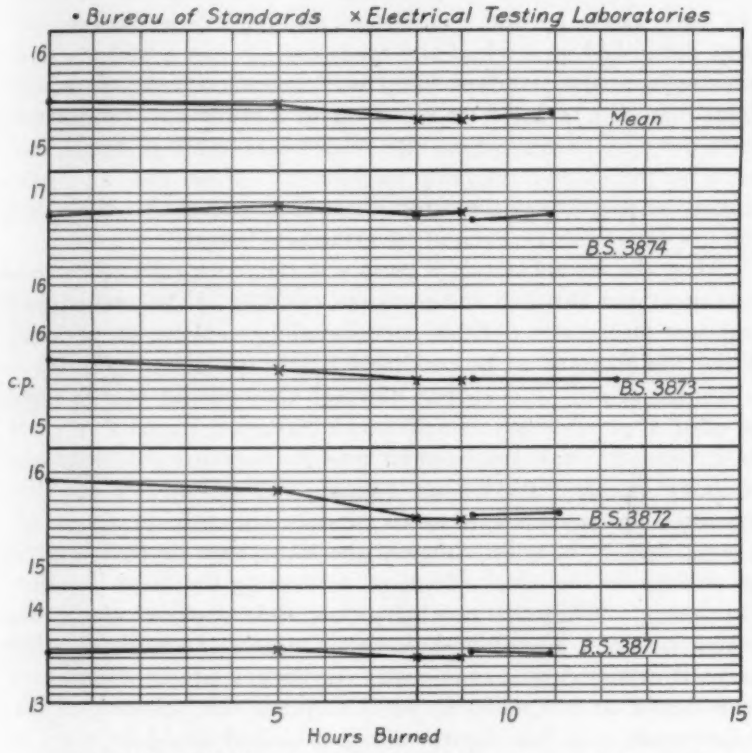
The process of making a photometric setting when melting the platinum cylinder consisted in placing the comparison lamp to give an equality setting of the photometer with the platinum somewhat below its melting point, and then slowly increasing the current through the platinum, moving the comparison lamp continually to preserve the photometric match until the instant of melt. It is obvious, that, in using this *method of approach* in setting, there is an opportunity for a considerable personal equation unless the standard lamp settings are made in the same way. This was accomplished by moving the standard lamps slowly toward the photometer and abruptly cutting off the light when a definite illumination was reached. In order to provide for this a special shutter was made which dropped when the standard lamp carriage reached a certain definite point and made electrical contact through a circuit which actuated a releasing trigger.

All the measurements were made in terms of a set of standard lamps which were kindly furnished by Mr. E. C. Crittenden, of the Bureau of Standards. These were single-loop, carbon-filament lamps used in the stationary position and requiring approximately forty volts for operation. The operating current was furnished by a set of local storage batteries, and voltages were measured by a potentiometer. The standard lamps were found to change slowly during the course of the work, and were, accordingly, sent several times to the Electrical Testing Laboratories for recalibration as well as twice to the Bureau of Standards. The values yielded by several calibrations are plotted in Fig. 27. In reducing the observations interpolated values were used, proportional to the time of burning of the standards since their last measurement.

(*c*) *Method of Performing Measurements.*—The general method of performing the measurements of brightness has been pretty well indicated by the description of the apparatus. It may, however, for purposes of greater clearness, be described here in detail by means of a typical measurement as actually carried through. This procedure was the one finally arrived at after various refinements developed during the course of the work; and was calculated to minimize the observational errors inherent in the method of approach which was necessarily used in making the settings.

The platinum cylinder was first placed in position in its holder. The platinum was then brought up to incandescence and its image observed on the photometer screen. The holder was then turned, if necessary, by means of the rack and pinion, until the dark band

FIG. 27.



Candle-power of standard lamps as measured during the progress of the work.

opposite the opening was seen perfectly centrally in the slit image. When this position was obtained the cylinder was rotated until the dark band disappeared behind the edge of the opening, thus bringing a "black" portion opposite the slit; then the lens was moved laterally by means of the vertical and horizontal racks shown in Fig. 23 until the slit image on the photometer field entirely covered the contrast field and was centred vertically. The vertical adjustment once made remained valid throughout the melts.

The adjustment of the platinum cylinder being thus completed, the current through the cylinder was turned off and a standard

lamp placed on the track T_1. This lamp was placed at approximately 140 centimetres from the photometric field, was brought to its proper voltage, and then a photometric match was made by moving the comparison lamp on its track. Then with both lamps carefully held to their proper voltage the standard lamp was moved by hand slowly toward the photometer, and a photometric match was maintained by the continuous motion of the comparison lamp, controlled by a wheel and cord passing over a pulley. This movement of the standard lamp was at such a rate as to take about one minute for it to move up to a point 120 centimetres from the photometer, where the photometer carriage made contact with the shutter releasing mechanism. Upon the fall of the shutter the position of the comparison lamp was read. Five such settings were made on each of two standard lamps.

After the second standard lamp was removed, the current was once more put through the platinum cylinder. The comparison lamp was placed at a position corresponding to about 70 per cent. of the illumination to be produced by the platinum cylinder at its melting point. The current through the cylinder was brought up until a photometric match was obtained. From this point the current through the cylinder was then varied smoothly and regularly (except insofar as voltage fluctuations on the line interfered) at such a rate as to produce the melt in about one minute, that is, in approximately the same time as that used in the settings on the standard lamps in covering the same illumination change. While this was being done the comparison lamp was moved so as to maintain a photometric match. Its position when the melt occurred was then read. Cylinders of the dimensions used melted at about 26 amperes through the primary of the transformer, and this figure once established could be used as a guide in the preliminary stages of the heating.

Following the melt two more series of five settings each were made upon the remaining two standard lamps. From the measurements on the standard lamps four sets of values for the comparison lamp were obtained. The mean of these four sets was then used in the formula derived in an earlier section, giving the brightness of the platinum black body as the final result. The complete figures for a typical melt follow:

Melt 8 c.

Mean of 5 settings on lamp 3871 [13.50 cp.]	120.62 cm.
Mean of 5 settings on lamp 3872 [15.50 cp.]	112.09 cm.

Mean of 5 settings on lamp 3873 [15.50 cp.]	112.29 cm.
Mean of 5 settings on lamp 3874 [16.77 cp.]	107.94 cm.
Setting on melt	111.35 cm.

Distance of standard lamps from photometer field = 120 cm.

hence, candle-power of comparison lamp is:

as derived from measurement of 3871, $13.50 \times \left[\frac{120.62}{120}\right]^2 = 13.648$

as derived from measurement of 3872, $15.50 \times \left[\frac{112.09}{120}\right]^2 = 13.524$

as derived from measurement of 3873, $15.50 \times \left[\frac{112.29}{120}\right]^2 = 13.572$

as derived from measurement of 3874, $16.77 \times \left[\frac{107.94}{120}\right]^2 = 13.577$

Mean $C = 13.580$

Formula: $b = \frac{C}{d_s^2} \times \frac{M^2 d_t^2}{a\,t} = \frac{C}{d_s^2} \times \frac{(4.968)^2 \times (43.25)^2}{1.1274 \times .8878} = \frac{C}{d_s^2} \times 50488$

$$\therefore b = \frac{13.58}{(111.35)^2} \times 50488 = 55.30$$

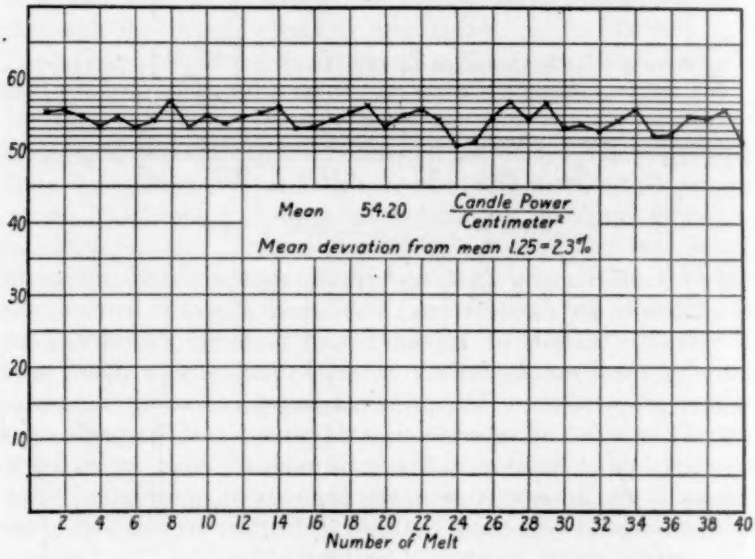
(*d*) *Experimental Results.*—In all, approximately 150 melts were made in the development of the method and in ascertaining the necessary details of apparatus and platinum cylinder form. Many of these measurements were in small groups made upon commercial platinum. The principal measurements by the visual method consisted of several series of 30 to 40 melts made upon chemically pure platinum. Study of each of these series led to changes in the apparatus or in the methods of observation; leading finally to the procedure outlined in the last section and to the experimental results which will be described as the "final series."

Of these preliminary measurements it will be sufficient to give the results of one of the earlier ones, pointing out the defects of apparatus and methods which it indicated. Fig. 28 shows the results of 40 melts made upon chemically pure platinum cylinders .075 millimetre in thickness. In this melt the values for the brightness vary between 50.6 and 56.7 candle-power per square centimetre. The mean deviation from the mean amounts to 2.3 per cent., with the probable error of a single observation equal to 2 per cent. The scattering of these values is altogether too high for a standard; and the mean value, namely, 54.2 candle-power per square centimetre, is, because of the large errors of the observations, probably of small value.

A study of the figure shows that the values are unsymmetrically placed around the mean. There are much greater excursions downward than upward. These suggested that, while the upper values were probably near the true figure, the occurrence of poor electrical contact, local variations in thickness of the platinum, and deformations of the cylinders, caused many melts to occur at lower brightness than corresponds to the true melting temperature.

Following this series another was made in which the thickness

FIG. 28.



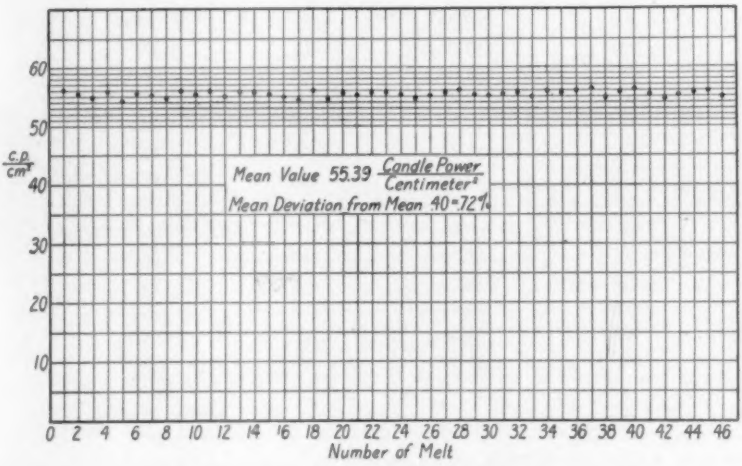
Results of series of melts on .075 mm. platinum.

of platinum was increased to .125 millimetre. With this series the average deviation from the mean was reduced to 1.33 per cent. However, the values below the mean were as before more widely scattered than those above it and therefore did not meet the reasonable criterion that the errors of setting should be evenly distributed about the mean. In these two series the platinum was clamped by single pairs of clamps such as are shown in Fig. 19. It was suspected that one of the chief causes of the outstanding scattering of values was insufficient electrical contact between the cylinder and its supports, a source of error which becomes more

serious the greater the thickness and stiffness of the platinum. In the next series, therefore, not only was the platinum made still thicker to prevent deformations which were still occasionally observed in the second series, but the double pairs of clamps shown in Fig. 15 were adopted. These clamps were very carefully made with specially constructed dies, and grip the platinum so firmly that all possibility of contact trouble is eliminated.

The final series of visual measurements consisted actually of

FIG. 29.


60
50
c.p./cm²
40
30
20
10
Mean Value 55.39 Candle Power / Centimeter²
Mean Deviation from Mean 40=.72%
0 2 4 6 8 10 12 14 16 18 20 22 24 26 28 30 32 34 36 38 40 42 44 46
Number of Melt

Results of final series of visual measurements.

two sets, one of 35 melts made upon cylinders .18 millimetre thick, of Baker and Company chemically pure platinum, and a shorter one consisting of 11 melts of cylinders of .20 millimetre thickness from platinum specially purified in our own laboratories as above described. The mean results of these two series differ by much less than their probable errors and have, therefore, been treated as a single group of 46 melts.

The results of these 46 melts are given in Table I and are shown plotted in Fig. 27. The *mean value* obtained for the brightness of the black body at the melting point of platinum is *55.39 candle-power per square centimetre.*

TABLE I.

Visual Observations.

Obs.	Value.	Obs.	Value.
1	56.11	24	55.35
2	55.40	25	54.67
3	54.82	26	55.06
4	55.89	27	55.60
5	54.27	28	56.11
6	55.62	29	55.16
7	55.39	30	55.04
8	54.81	31	55.38
9	55.98	32	55.65
10	55.34	33	54.91
11	55.98	34	55.84
12	55.04	35	55.35
13	55.87	36	55.85
14	55.77	37	56.23
15	55.47	38	54.69
16	54.97	39	55.53
17	54.50	40	56.35
18	56.01	41	55.30
19	54.70	42	54.56
20	55.22	43	55.15
21	55.17	44	55.55
22	55.66	45	55.90
23	55.72	46	54.98

Mean, $55.39 \frac{\text{cp.}}{\text{cm.}^2}$

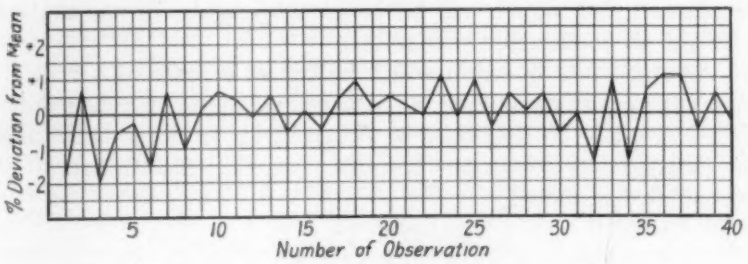
(*e*) *Analysis of the Results.*—In order to arrive at a fair estimate of the accuracy of this result, the probable errors in the various elements contributing to the final value should be studied. These may be divided into three groups: First, the errors in the determination of the physical constants of the apparatus; second, the errors due to the method of photometric observation, specifically the method of approach; third, the uncertainty in the mean result inherent in the necessary limit to the number of observations.

Taking first the constants of the apparatus, namely, the various measurements of lengths, the magnification, the transmission of the lens; these are each determined to within 0.1 per cent. The standard lamp intensities, which fall in the same category of constants, were estimated by the Bureau of Standards insofar as their own determinations were concerned to be each

reliable to 0.2 per cent. and the mean of the four lamps to 0.1 per cent.

The errors due to the method of observation and to the limited number of observations may conveniently be considered together. Taking the final results and tabulating their deviations from the mean, we find for the *mean deviation,* 0.72 per cent. Calculating the probable errors by the method of least squares we find, for the *probable error of one observation, .62 per cent.* for the *probable error of the result, 0.1 per cent.* Before estimating the probable errors due to all causes, it is of interest to separate out the errors due to the method of photometric setting employed. This is made possible by a separate study which was made of the errors of

FIG. 30.



Series of photometer settings by the method of approach.

setting, by the method of setting by continuous approach, using a carefully controlled standard lamp as the object of measurement. In Fig. 30 are shown a series of 40 settings made under the conditions used in the investigation, plotted in terms of their deviation from the mean. It will be seen that the settings vary in this series over a range of 3 per cent. The mean error of setting for this particular series was .65 per cent. Several other series were made, after greater practice had been obtained, yielding somewhat lower mean errors. Finally, an analysis was made of the settings on the standard lamps made throughout the final series of observations. On these the *average deviation from the mean was found to be 0.46 per cent.* Using this figure in conjunction with the total number of melts (46) in the formula for determining probable error (Peters' approximation formula was used), we find that *the portion of the probable error in the result*

which can be ascribed to the method of photometric setting is 1/20 per cent.

The value just obtained for the average error due to inaccuracy in the photometric setting enables us to estimate the average deviation of the true brightness of the melting platinum from its mean value. For this purpose we use the formula:

$$\Delta_t = \sqrt{\Delta_1^2 + \Delta_2^2}$$

where Δ_t is the mean error of the results, and Δ_1 and Δ_2 are the mean errors of the melt brightnesses and the photometric observations. Inserting the actual values:

$$0.72 = \sqrt{\Delta_1^2 + 0.46^2}$$

we get $\Delta_1 = 0.56$ per cent.

This corresponds to somewhat less than one degree in temperature. This figure will be referred to again in conjunction with the photo-electric observations.

An important question which will arise in case of a redetermination is as to the number of meltings necessary to a determination. A guide to this may be obtained by working backwards in Peter's approximation formula for the determination of probable error, namely:

$$R = \frac{.8453 \times \Sigma(+v)}{n\sqrt{n-1}}$$

where R is the probable error, n the number of observations, and $\Sigma + v$ the sum of the absolute values of the errors. By inserting the product of the number of observations by the mean error, that is, $n\bar{v}$ in place of $\Sigma\ (+v)$ and inserting the error tolerated in the result in place of R, the probable error, and solving for n, we get:

$$n = 1 + \left(\frac{.8453\ \bar{v}^2}{R}\right).$$

Carrying through this process for $R = 0.2$ per cent. gives $n = 10$. Several random selections of ten observations show this figure to be approximately correct. A rather larger number would, however, be indicated from a survey of the results as safer. *As a round number fifteen observations are recommended as adequate to secure a result correct to 1/5 per cent.*

Reviewing now all sources of error, it appears that there are none which should exceed 0.1 per cent. There is no reason why these should all be in one direction, so that the chance of their

mounting up to more than 0.2 per cent. is remote. It therefore appears reasonable to assign to the result an accuracy of 0.2 per cent., so that it may be stated as:

$$b = 55.39 \pm .11 \text{ candle-power per square centimetre.}$$

It should be noted that this estimate of error does not refer to the possible deviation of the result from the figure for an ideal black body, but only to the error in the determination of the brightness of the platinum black-body structure as used. The degree of approximation to the proposal of Waidner and Burgess will be discussed later.

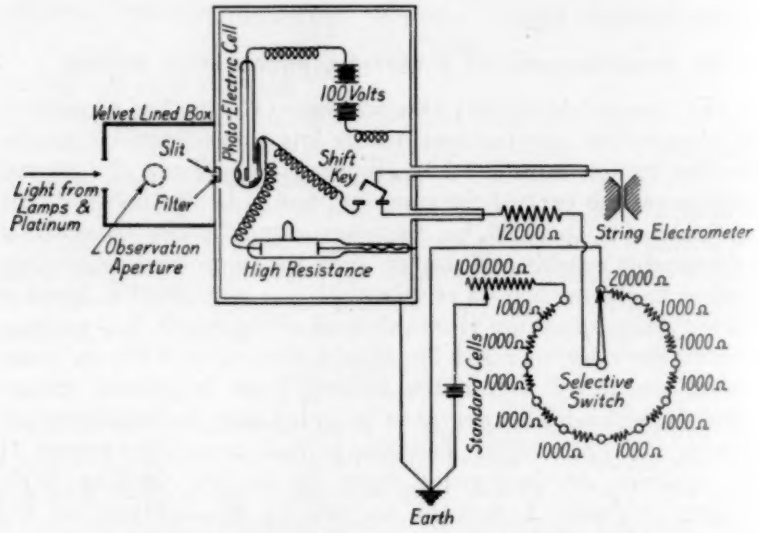
IV. MEASUREMENTS BY A PHYSICAL PHOTOMETRIC METHOD.

(*a*) *General Method of Observation.*—The method of making the photometric observations which has been described in the previous section is believed to eliminate any effects of personal equation on the part of the observer, and it is also believed that the final value obtained for the phenomenon under observation is thoroughly reliable. However, since the mean error of setting, peculiar to the method of observation, is a considerable fraction of the average deviation from the mean of the result, it is unquestionably desirable to make the observations if possible by some method which will record the melting point brightness instantaneously with a higher degree of accuracy than the visual method permits. It is also highly desirable to have a complete record of the variation of brightness right up to the melting point in order to make a detailed analysis of the behavior of the proposed standard.

These several requirements it was thought might be met by the use of a physical photometric method of observation employing a photo-electric cell. The second large series of the observations, which form the subject-matter of this section of the paper, was made by such a method. The method may be described in general as the measurement and automatic recording of the current from a photo-electric cell passing through a measuring instrument of high speed of response. In order that this method may be exact, certain requirements as to the nature of the response of the measuring system must be met. First of all the response of the photo-electric cell as a function of wave-length should correspond to that of the average eye. It is easy to fulfill this requirement

adequately in the present case owing to the exact color match which exists between the black body at the temperature in question and the carbon lamp standards. It is only necessary to have a rough approximation to the luminous efficiency curve of the spectrum. The second requirement is that the measuring instrument must be as nearly instantaneous as possible. For this purpose it was decided in the present work to use a string electrometer so connected as to measure the potential drop across a high

FIG. 31.



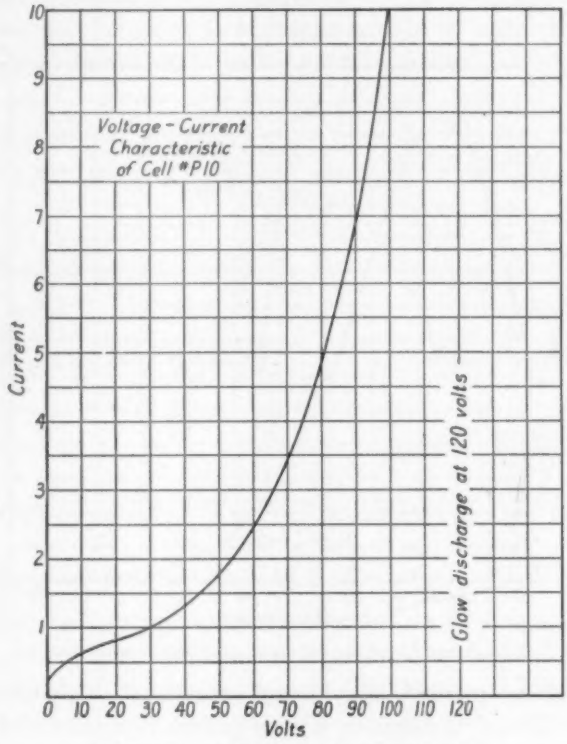
General scheme of photo-electric cell and string electrometer.

resistance in series with the photo-electric cell. The deflections of the string electrometer were projected upon a moving photographic sensitive paper which after development could be measured. The feasibility of the method depends upon the quickness of response of the measuring system, upon the magnitude of the deflections which can be obtained, and upon the accuracy with which these can be measured.

(*b*) *Apparatus.—Photo-electric Cell:* The photo-electric cell used in this investigation was prepared according to the general method developed by Elster and Geitel. The form of the cell is shown in Fig. 31. The active metal was potassium, which

was distilled on to the walls of the bulb and subsequently colored by the passage of a glow discharge in hydrogen. The hydrogen was then pumped out and argon at a low pressure introduced. The voltage-current characteristic of this cell is shown in Fig. 32. The illumination-current relationship is closely linear, although,

FIG. 32.


Voltage - Current
Characteristic
of Cell #P10
Current
Volts
Glow discharge at 120 volts
0 10 20 30 40 50 60 70 80 90 100 110 120
1 2 3 4 5 6 7 8 9 10

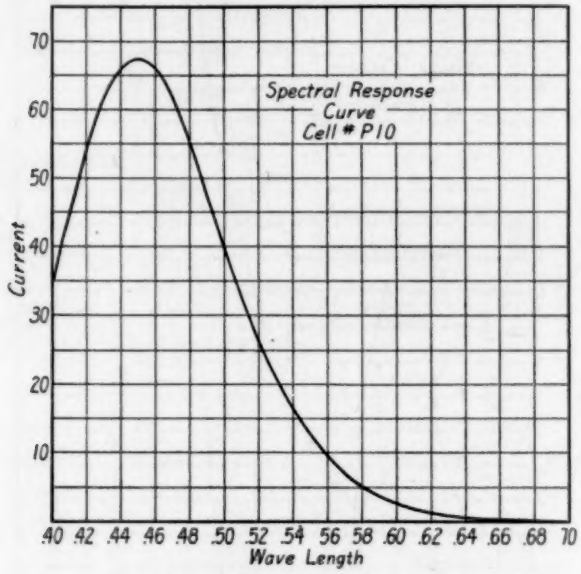
Voltage-current relation in photo-electric cell.

as will be seen, this characteristic is not required in the method used. The cell was selected for use from a large number which were made up for this and other purposes. In selecting these cells measurements were made of their distribution of response through the spectrum by means of a monochromatic illuminator, using a light source of known energy distribution. From these cells, the one which was most sensitive in the long wave region was selected. Its distribution of response according to wave-length is shown

in Fig. 33. At wave-length .55μ this cell was about twenty-five times as sensitive as any one of a group of half a dozen photo-electric cells purchased from an apparatus supply house.

It is possible by combinations of absorbing media to obtain a response curve from a cell such as the one selected which is very close to the characteristic of the eye. This is accomplished, however, at the cost of a considerable loss of light and in the present

FIG. 33.



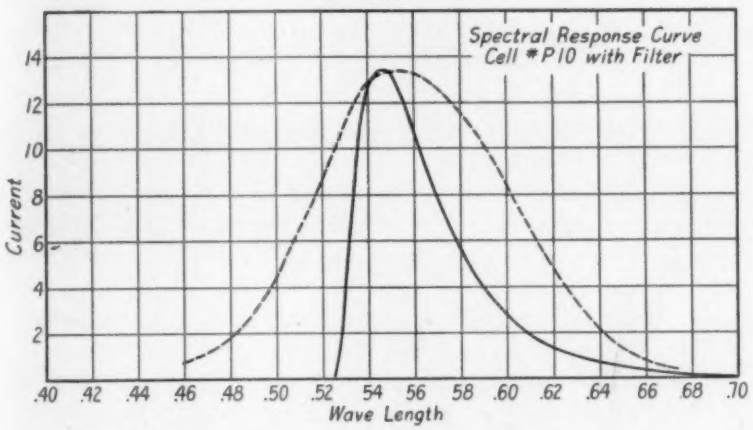
Wave-length distribution of response in photo-electric cell.

case, as above pointed out, is not necessary. The filter finally used with the cell consists of a dyed gelatin film mounted between two plain glasses; the film being colored with a yellow dye—tartrazine—having a very abrupt cut-off in the blue. The response curve of the cell in conjunction with this screen is shown by the continuous curve of Fig. 34; its maximum is close to that of the visual response curve (the dashed curve) at .55μ, but is considerably sharper. It is, however, considered an entirely adequate approximation for the purpose, in view of the negligible color difference between the black body and the standard lamps.

The photo-electric cell was mounted in a box at the end of the

photometer track previously occupied by the special photometer field described earlier in the paper. The light from the standard lamps and from the platinum fell upon an aperture of dimensions 2×10 millimetres cut in thin sheet metal. The width of this aperture is a little less than half the width of the image of the black-body opening, so that there was ample margin on either side. Its length corresponds to 2 millimetres in the cylinder, which is just about the length which disappears in a melt, and so is presumably all raised to the melting temperature. Directly behind

FIG. 34.



Wave-length distribution of response in photo-electric cell with filter.

and against the aperture was placed the yellow filter with its front face ground so as to diffuse the light to the photo-electric cell, which was mounted almost but not quite touching the filter, as shown in Fig. 31. In the box with the photo-electric cell was a battery of dry cells from which 100 volts could be obtained if desired. One terminal of the batteries was earthed to the wall of the box, and the cell was connected to earth through a high resistance composed of 90 per cent. xylol and 10 per cent. alcohol in a sealed glass tube with one movable electrode, as shown in the figure. From the common point of the photo-electric cell and the high resistance a wire led to a mercury cup forming part of a key system worked from outside the box, by means of which the cell could be connected directly to the fibre of the string elec-

trometer. The box containing the photo-electric cell was hermetically sealed with a tray of phosphorous pentoxide standing in it for drying purposes. The wire leading to the electrometer passed over quartz insulating supports out through a short pipe of small diameter to a terminal of the electrometer.

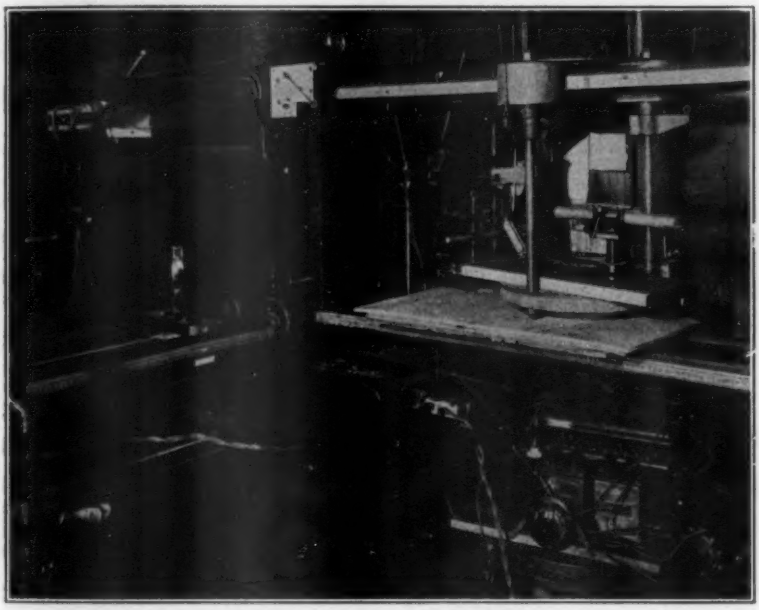
Electrometer: The string electrometer used was obtained from the Cambridge Scientific Instrument Company and connected up according to the instructions given by the maker. The voltage-deflection characteristics of this instrument depend upon the adjustment, as does also the quickness of response. With different adjustments of tension and of the position of the fibre, the voltage-deflection relationship may be altered from a convex to a concave curve. It is also characteristic of the instrument that for high sensibility the adjustment is unstable, so that the magnitude as well as the character of the deflection will change much too quickly to be depended upon in any series of observations. This means that the instrument must be used only in a strictly substitution method of observation. The sensitiveness which may be successfully used depends upon the potential which is available for measurement and upon the speed of response which is required. In this work the adjustment selected was such that the deflection of the projected image of the fibre at one metre distance was approximately 2 centimetres per volt, using a microscope projection lens of 16 millimetres focal length. With this adjustment the complete deflection is assumed in a time which is of the order of magnitude of .01 second.

Because of the continuous drift of the electrometer characteristic, it is quite necessary to have standard reference marks. These were arranged for by a potentiometer system consisting of two standard cells in series with ten high resistances with their terminals brought out on to a dial. By shifting from one point to another on the dial various voltages could be impressed on the electrometer. By means of an additional resistance in series with those brought out to the dial, the absolute magnitude of the voltage steps could be regulated to fit the voltage available from the photo-electric cell. In order to prevent building vibration being transmitted to the electrometer fibre, the electrometer with its plate batteries were suspended on a modified Julius suspension. The complete photo-electric cell, electrometer and reference voltage

system is shown diagrammatically in Fig. 31, and by the photograph, Fig. 35.

The position of the electrometer fibre was recorded photographically on a moving photographic sensitive paper fed past a narrow slit at right angles to the long dimension of the fibre image. A time record was simultaneously made by the shadow

FIG. 35.



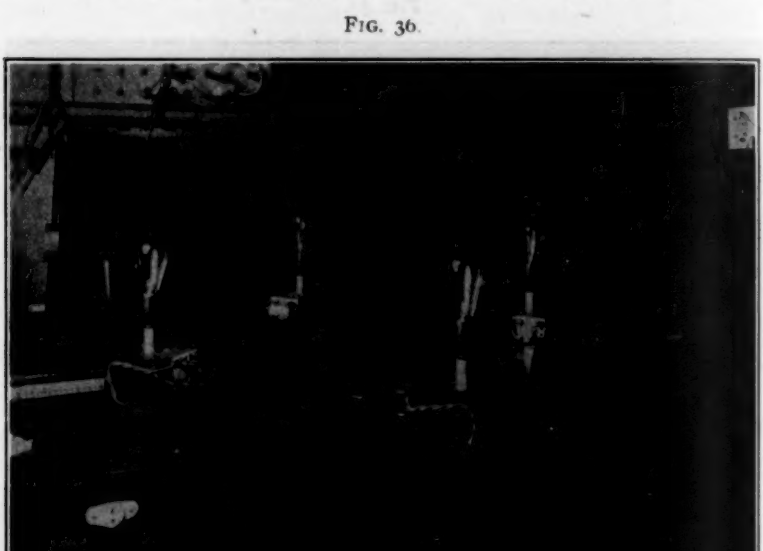
Photograph of photo-electric cell box, string electrometer, and accessories.

of a pointer actuated by a large swinging pendulum with a period of approximately three seconds.

Mountings for the Standard Lamps: The four standard lamps were mounted for this work on a turn-table which is shown in Fig. 36. This arrangement was made partly in order to reduce to a minimum the time necessary for shifting from one lamp to another and also to serve as a simple means of approaching the lamps to their proper position slowly and smoothly in the manner which, as will be described below, was found advisable in making the observations.

Miscellaneous Changes in the Apparatus: In setting up the apparatus for making the observations in this way the opportunity was taken to make several changes which were suggested by the earlier work. One change was in the apparatus for holding the platinum cylinders. This consisted in substituting for the rigid rod holding the top clamps to which cooling vanes had been attached, a hollow T-shaped piece of Invar. The purpose of this

FIG. 36.



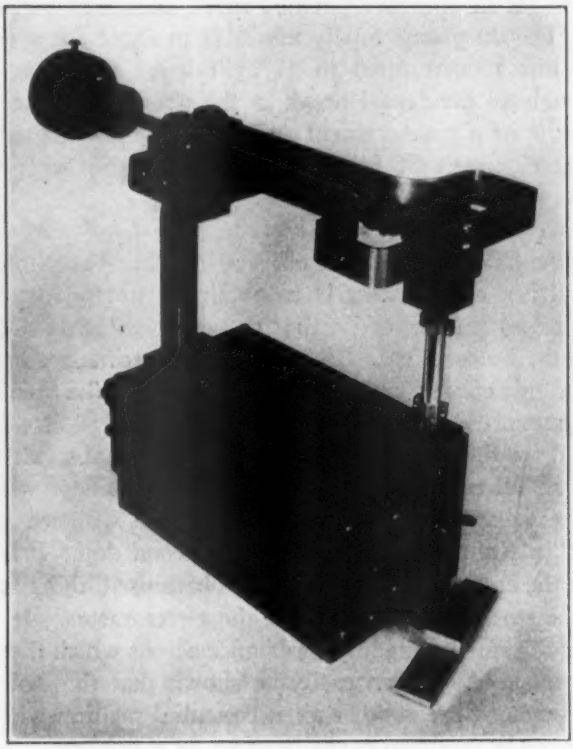
Turn-table mounting for standard lamps.

was first of all, by the shape of this support, to provide that the thermal expansion would merely spread the parts of the support laterally instead of longitudinally so as to leave the attached end of the platinum cylinder stationary. In addition, by making the support of Invar, the amount of change of size was reduced to a minimum. This change in construction is shown in Fig. 37.

Another change which was made was in the length of the opening in the platinum cylinders. Since the point of measurement is by this method necessarily fixed, there is no advantage in having the opening in the platinum black body greatly longer

than would correspond to the aperture in front of the photo-electric cell. The platinum cylinders were accordingly, in this part of the study, given an opening only 1 centimetre long. The opening in the cylinder was placed, as the result of several trial melts,

FIG. 37.



Final form of platinum cylinder holder.

3 millimetres above the midpoint between the clamps. With the improved apparatus the position at which the melt occurred was very closely the same every time. The actual position at which the melt occurred could be observed through an observation hole in the side of the shielding cylinder on the photo-electric cell box. If the melt occurred badly off centre, or off the end of the aperture, as did happen in some cases, the datum was discarded. In the

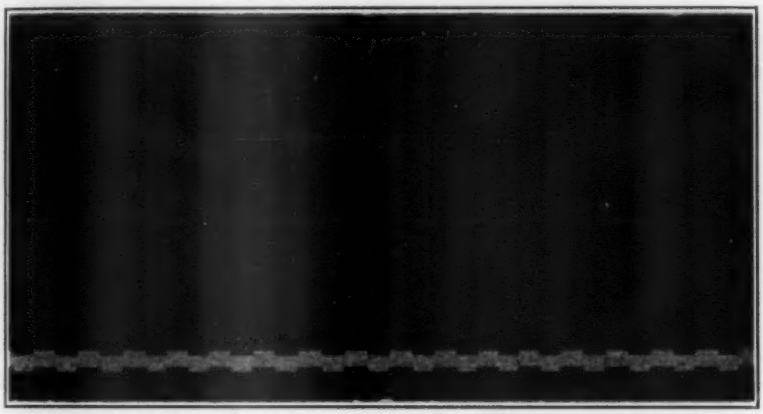
visual observations it was possible to concentrate attention on the brightest part of the cylinder image, so that small deviations in the portion of melting made no difference. The photo-electric method is thus more wasteful of platinum, but the other advantages of the method entirely outweigh this objection.

The Platinum: The platinum used in these measurements was the second lot of specially purified metal described earlier in the paper. The 500 grams finally available in sheet form (of thickness .18 min.) correspond to 31 cylinders. Of these one was lost through an accidental break in the photo-electric cell circuit in the midst of a measurement and two were not melted because of the development of blisters on the platinum surface in the preliminary annealing operation.

(*c*) *Method of Making Observations.—Choice of Electrical Conditions:* One of the chief objects in using the photo-electric cell and string electrometer is to obtain as nearly instantaneous readings as possible in order that the exact value at the time of melting may be recorded. In order to approximate as closely as possible to this condition, it is necessary to study the time relations of the system. First of all the photo-electric cell itself may be considered for the present purpose as responding absolutely instantaneously. When, however, a measuring instrument which must be charged is attached to the cell some time will necessarily be consumed in attaining a steady indicating condition. It is obvious that with the arrangement of apparatus here used, this time will be shorter the smaller the capacity of the electrometer. It will also be shorter the smaller the high resistance across which the potential drop is measured. It can easily be shown that the deflection of the electrometer fibre follows an exponential relation with time in which the resistance and capacity enter as a product. Assuming the capacity of the electrometer fixed, it is desirable to work with as small a series resistance as possible, which means, if the amount of light available is also fixed, as it is in this case by the dimensions of the optical system and the size of the image of the platinum black body, that the photo-electric cell should be as sensitive as possible and as little should be sacrificed by the color filter as is possible without endangering the proportionality of the response to the visual value.

One means of securing a large photo-electric current is to use as high a voltage as is safe on the photo-electric cell. For as is shown in Fig. 32, the current increases very rapidly with voltage as the breakdown voltage of the cell is approached. There is, however, a limitation to the use of the higher voltages set by the fact that, in this type of cell, as the electrometer charges up, the voltage drop across the cell itself is decreased, with a consequent drop in the current. This drop in current occurs simultaneously with the growth of the voltage on the electrometer needle, and

FIG. 38.



String electrometer record exposure to steady illumination showing "over-shooting" due to the use of too high a voltage in the photo-electric cell.

the result, if the battery voltage is high, may be an over-shooting of the deflection. This is illustrated by Fig. 38, which is a copy of an actual record obtained with 120 volts on the cell. This over-shooting effect is avoided by working at a somewhat lower voltage where the alteration in current is much less for any given change in the voltage drop across the cell. It was found by experiment that a voltage of 90 resulted in a voltage record which did not over-shoot.

Using the voltage value selected as just indicated the xylol-alcohol resistance was adjusted so that the illumination corresponding to the platinum black body at its melting point gave approximately one volt drop across the resistance. It was found possible under this condition to use the electrometer with the

very rapid response characteristic previously mentioned. Under these conditions the photo-electric current utilized is approximately 10^{-10} amperes. The time taken for the deflection to attain its full value, corresponding to approximately one volt, is less than one second; but 95 per cent. or more of the deflection is attained in less than 0.1 second, and hence any slowly changing illumination would probably be followed with great accuracy. In order to test this point, records were made by the photographic recording apparatus of the illumination from standard lamps both when the lamps were standing stationary and when the lamps were slowly moved to position; the light being abruptly cut off in each case by a falling shutter. Measurements of the records showed a slightly but consistently lower value when the illumination was still changing at the instant of obscuration, corresponding to about ½ per cent. in the calculated illumination. In order to eliminate this error, it was decided to cause the standard lamps to approach their proper positions slowly so as to simulate the increasing illumination which results from the gradual increase of temperature of the platinum up to the melting point. For this purpose the lamps mounted on their turn-table, as shown in Fig. 36, were each in turn swung slowly from a position where they just illuminated the photo-electric cell through the openings of the screens, around to a stop. At this stop was placed a buzzer contact which served to indicate when the lamp reached its final position. It was then swung back quickly; the whole process resulting in a very close copy of the average behavior of the light from the platinum black body. This method will be recognized as similar in principle to that used in the visual observations and it is of course done with the same general object. All necessity for modifying the procedure of making measurements in order to allow for the lack of instantaneity of the response of the system would be overcome by an increase of the electrometer sensibility of about ten times. Such an increase would be easily possible in an electrometer designed specially for this work. An increase in the sensitiveness of the photo-electric cell by the same factor, as might be accomplished by the substitution of rubidium for potassium, would also be effective in this way.

Measurement of the Records: A complete record consists, besides the deflection due to the melt, of the deflections from

four standard lamps taken before the melt and of the same lamps taken afterward. Between each record from the photo-electric cell is inserted a set of voltage steps. A portion of a typical record is shown in Fig. 39. On this are shown four sets of voltage steps, between the first and second of which is the record of one standard lamp; between the second and third that of the melt; between the third and fourth that of another standard lamp. At the bottom is shown the time record controlled by the pendulum, while through the centre of the record is a white line made by the shadow of a wire across the slit, which is used for a reference line in measuring the records.

The records were measured by means of a Gaertner micrometer microscope, the paper strip being folded under a glass plate. The microscope was moved to successive measuring positions until the group of voltage steps or photo-electric cell current values, exposed by the mere movement of the microscope, were covered, after which a new section of the record was folded under and the process repeated. The definition of the projection system and the fineness of the fibre were such that there was no difficulty in measuring the records with an accuracy corresponding to one-tenth of 1 per cent. in the illumination of the photo-electric cell.

FIG. 39.

Portion of typical record of a platinum cylinder melt.

The measurements thus obtained were next plotted on large accurate coördinate paper, using times as abscissæ and the measured values of the deflections as ordinates. The points corresponding to the various voltage steps were then joined up by smooth curves. The next step was to read off the voltage values from these curves at the times corresponding to the lamp and melt observations. These deflections were plotted against the voltage step values, and by interpolation the "voltage values" of the lamp and melt illuminations derived. A final plot was then made of illuminations, taken from the candle-power values of the standards, against voltage values, and the illumination of the platinum black-body image read off the resultant curve. This was then immediately reduced to candle-power per square centimetre by use of the formula:

$$b = \frac{I_p M^2 d_1^2}{at}$$

where I_p is the illumination due to the platinum and the other symbols are identical with those previously given.

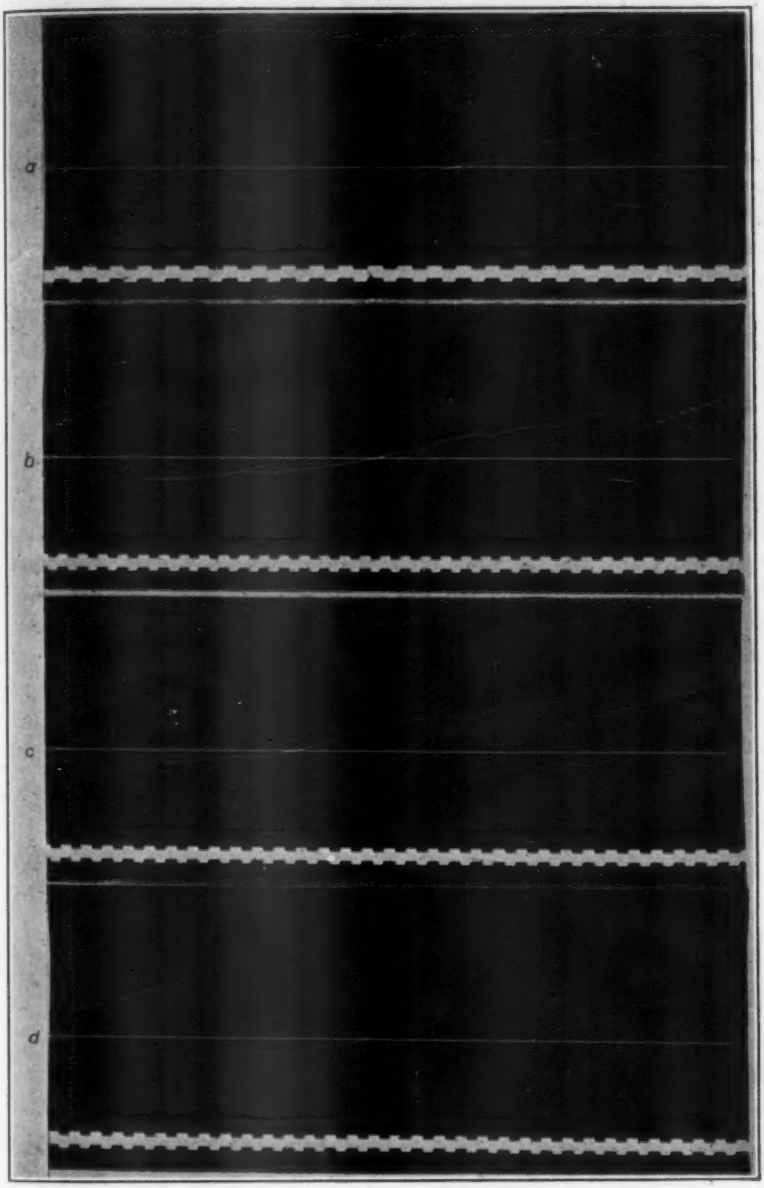
The accuracy obtainable by this plotting and interpolation process is very satisfactory. It is greatest where the electrometer adjustment remains relatively stable, as in this case the curves connecting deflection with time are very closely straight lines. The method is also most accurate when the relation between voltage and deflection of the electrometer is most nearly rectilinear. These conditions were very closely adhered to in all the work. An idea of the reliability of the observations may be obtained from the voltage values which were found corresponding to the standard lamps as run before and after the melt. A representative set of these values is as follows:

Lamp	3871	3872	3873	3874
Volt step value before melt.....	2168	2429	2443	2614
Volt step value after melt.....	2156	2429	2442	2616

In the majority of cases the values before and after did not differ by more than five parts in a thousand.

(*d*) *Results of the Photo-electric Observation.—Characteristics of Records:* One very great merit of the photo-electric method of observation using photographic records is that the behavior of the platinum right up to the instant of melt is recorded for analysis. Inspection of the series of records obtained in this study reveals some points of interest. Several representative records of melts are shown in Fig. 40. It will be seen from these that

FIG. 40.



Representative melt records.

under some circumstances (which were in the great majority) the platinum increases in brightness uniformly up to the melting point and that the latter point corresponds to an abrupt drop in brightness (record *a*). On the other hand, some records show fluctuations in brightness superposed on the slow rise (record *d*). These are perhaps to be ascribed to fluctuations in the alternating current used to heat the platinum. Such erratic fluctuations occasionally occur, as is shown by a voltmeter connecting to the line. While the majority of the melts occur at the attainment of the maximum brightness, in several cases the brightness remained constant for several seconds before the melt occurred (record *b*). In one case shown (record *c*) the brightness fell off somewhat from a maximum; the melting occurring about one second after the maximum brightness was attained. This is probably explained by a crumpling or flowing of the surface preceding the final rupture. Reviewing the whole series of records, it may be said that while in the majority of cases the melting points of the cylinders appear sharp and well defined, there are enough exceptions to this to indicate that the point of collapse of the cylinders is actually subject to some uncertainty such as would be caused for instance by surface tension holding them together after the metal had actually melted. No correlation between the character of the melt and the corresponding brightness value has been noted.

Values Obtained.—The values obtained for the brightness of the platinum cylinder black bodies at their point of failure are listed in Table II, and are shown graphically in Fig. 41. Of the

TABLE II.

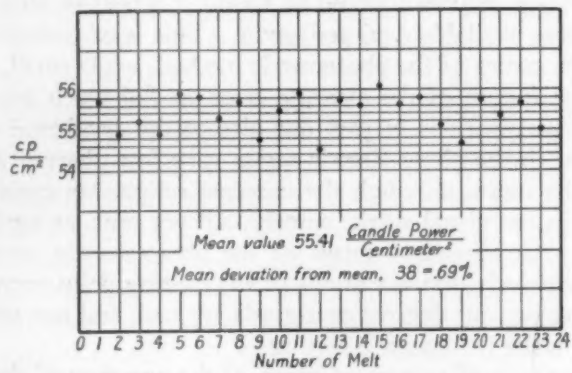
Photo-electric Observations.

Obs.	Value.	Obs.	Value.
1	55.72	13	55.72
2	54.91	14	55.60
3	55.21	15	56.07
4	54.91	16	55.62
5	55.92	17	55.77
6	55.82	18	55.11
7	55.31	19	54.66
8	55.77	20	55.72
9	54.76	21	55.36
10	55.46	22	55.67
11	55.92	23	55.01
12	54.50		

Mean, 55.41 $\frac{\text{cp.}}{\text{cm.}^2}$

29 melts made, six were discarded because the melt occurred in such a position that its image fell to one end or entirely off the aperture before the photo-electric cell. The reasonableness of discarding these values is unquestionable, although from the results it is rendered unlikely that the majority of these off-centre melts were really in error. It is probable that while a melt whose image does not fall on the aperture *may* be in error, particularly if the rise to the melting temperature was made quite rapidly, on the other hand, the value may be perfectly good if the temperature has been rising so slowly that an extended portion of the cylinder

FIG. 41.


cp/cm²
56
55
54
Mean value 55.41 Candle Power/Centimeter²
Mean deviation from mean, .38 = .69%
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24
Number of Melt

Results of photo-electric series of measurements.

is at uniform temperature. Actually the mean value for all observations is only about one-fifth of 1 per cent. lower than the value from the unobjectionable observations. The principal result of throwing out the observations which fell off centre was the elimination of one determination which was rather badly out of line, being 3½ per cent. below the mean.

The mean value from the twenty-three unobjectionable determinations as calculated from interpolated values of the standard lamps derived from their calibration before and after the series, is

b = 55.41 candle-power per square centimetre.

(*e*) *Discussion of Photo-electric Observations.*—The photo-electric observations confirm the visual observation in a very gratifying manner. The photo-electric method of observation which is here used is susceptible of very high accuracy, as has been shown in a previous section, and suggests itself as worthy

of consideration in the intercomparison of photometric standards of any sort where there is no color difference to be contended with.

The photo-electric results when analyzed yield the following figures:

Mean deviation from the mean	0.69 per cent.
Probable error of a single observation	0.59 per cent.
Probable error of the result	0.12 per cent.

These figures are better than for the visual observations, with the exception of the last, which is not quite so good solely because of the much smaller number of observations. The mean deviation from the mean is not so low as the analysis of the visual method of observation indicated would be found if a perfect photometric method were available (.56 per cent.). This is of course reasonable, as the errors of the photometric method, while small, are not zero. The failure of the average error to fall much below that for the visual work is in part due also to the accidental circumstance that in the photo-electric series very few observations fell close to the mean, although the extreme range was considerably less than in the visual work, namely, 2.8 per cent. as against 3.8 per cent. An important result of the photo-electric work is to show conclusively that deviations of the melting point corresponding to three or four degrees centigrade are real, and not ascribable to errors in the photometric setting.

The analysis of sources of error, of the accuracy of the result, and of the number of observations necessary for a determination is substantially the same for the photo-electric as for the visual observations and need not be repeated.

V. GENERAL SUMMARY AND DISCUSSION OF RESULTS.

(*a*) *Final Experimental Value.*—The complete series of sixty-nine observations is listed in Tables I and II, showing the visual and the photo-electric observations with the mean of each set, respectively. For the final value the visual and photo-electric series are given equal weight, leading to the following value for the brightness of the platinum black body at its melting point:

$$b = 55.40 \pm .11 \text{ candle-power per square centimetre.}$$

(*b*) *Discussion of the Investigation as a Whole.*—Upon comparing the results of this study with the earlier attempts to utilize the melting point of platinum (Violle, Siemens *et al.*) it is evident at once that the chief sources of error inherent in their methods

have been mastered. The elimination of contaminable surface by the use of the black-body type of radiator, and the use of platinum of considerable thickness formed into a rigid mechanical shape have reduced the scattering of values found with the Siemens' standard—the nearest in form to that here studied—from " 10 per cent. and greater " as found by Lummer to an extreme range of less than 3 per cent., and the mean error of setting from 2.9 per cent. as found by Liebenthal to .69 per cent.

It is now pertinent to compare the platinum black body with the existing luminous standards, that is, with the flame standards discussed earlier in the paper. In making this comparison it is desirable, and indeed necessary, to consider the new standard from two points of view: First, it is to be judged as a new kind of standard " lamp," that is, as a certain structure of platinum, which, when made up and operated according to specifications, has certain luminous properties; and, second, it is to be examined as a realization of the proposal of Waidner and Burgess which was the inspiration of the work.

Proceeding now to the comparison of the platinum black body with the existing flame standards, we are to compare, as to those properties desirable in light standards, the performance of a cylindrical form of black body made of platinum to certain specified dimensions, and observed at its melting point, with the behavior of the only practical flame standards, the Hefner and pentane lamps. The first point to be compared is the *precision of setting* possible with the two kinds of standards. Strictly comparable data are not plentiful; the most definite for our purpose are perhaps the series made by Liebenthal on the Hefner in studying the Siemens standard. His mean error of setting with a large number of measurements of two Hefner lamps against each other was .95 per cent., or considerably greater than the mean error in this work. In the extensive work of Rosa and Crittenden [16] on the Hefner and the pentane lamps, data on mean deviations from the mean are given for the final values after all corrections for atmospheric conditions were applied. They give for the Hefner lamp values from .44 to .74 per cent.; for the pentane lamp, an average of slightly less than .4 per cent. It is, however, necessary to point out that while in the platinum work each measurement is given by *a single setting*, a " measurement " in the work of Rosa and Crittenden is the mean of *twenty to thirty*

settings during which the flame was inspected and adjusted several times; the mean errors of single settings must necessarily have been very considerably larger than the figures above given. If the whole series of sixty-nine platinum melts included in the measurements made by both methods of observation are grouped at random into "measurements" of twenty settings each, the mean value of any such measurement will agree with any other similar mean to better than 0.1 per cent. From this comparison it is clear that the new platinum standard, considered from the standpoint of precision of setting, is notably better than the flame standards.

The comparison just made leaves out of account all considerations of time, material, and labor cost in making measurements and, while strictly accurate, might well be offset by economic considerations if other standard lamps were available which gave equally accurate results as the average of a large number of observations which could be quickly and easily made. Thus the twenty observations constituting a pentane lamp "measurement" can be made much more quickly than a single platinum melt, so that if the average constituting the measurement were from all other standpoints equally satisfactory with the platinum standard the absolute superiority of the single settings of the latter would not constitute a practical advantage. As will be seen, however, the existing flame standards are inferior on practically every other count as well. In regard to the cost of material, one might imagine at first sight that the large quantities of platinum used in a series of determinations would make the measurement extremely expensive. Such is not, however, the case. While the first cost, that is, the capital investment, is indeed considerable, the loss of metal is extremely small—about one-fifth of 1 per cent. in the work here reported—so that the chief element of expense is the working of the platinum, which only amounts to a small fraction of its metal value.

A great advantage of the platinum black body over any flame standard is its freedom from atmospheric influences, since both the pressure and humidity ranges in the atmosphere are altogether too small to affect the melting point of platinum to an observable extent. This results in simplicity of specification, since the specification of a flame standard is not complete until the formulas for its dependence on pressure and humidity are established and the

methods of measuring these variables are agreed upon. Furthermore, this independence of atmospheric conditions contributes to greater accuracy in the result. The need for determining and applying complicated corrections inevitably reduces the accuracy of a measurement, so that even were the atmospheric corrections to the flame lamps accurately known, and easily applied, which is hardly the case, the independence of the platinum standard of these corrections would still decidedly favor it.

The next point to be considered is the relative simplicity of construction and of the construction specifications necessary to secure reproducibility. In this respect the platinum black body is very much simpler than the pentane lamp. It is not so simple as the Hefner lamp, but the difference does not nearly offset the difference in precision of setting and in freedom from atmospheric conditions just discussed. Strictly speaking, it is not possible to state how accurately reproducible the platinum black-body standard lamp is, until other investigators have independently set up duplicates. There is, however, every reason to believe that it will be entirely reproducible, since all of the specifications, with the exception of that for the composition of the platinum, are in terms of lengths, and all the constants of the measuring apparatus are measured as lengths or as simple transmissions (image-forming lens). All the lengths involved can be measured to a degree of accuracy considerably higher than can be attained in photometric work. In regard to the chemically pure platinum, the results on three lots prepared at various times and by two different laboratories show no differences in melting point; indeed, the impurities occurring are so minute that no difficulty whatever is to be anticipated in this regard. In addition the methods of testing the platinum for purity are so delicate that the specification of the platinum is more exact than the specification of the fuel for a flame standard.

The last point to be considered is the suitability of the light from the platinum black body for accurate photometric measurement. In respect to color, the condition is, as already pointed out, ideal, because of the match with the carbon incandescent lamp maintaining standards. In regard to intensity the new standard leaves little to be desired; the projection lens aperture used in the investigation was equivalent just before the platinum melted to a 60 candle-power light source, so that an illumination of about

12 meter-candles was available on the photometer field. Less favorable to the proposed standard is the fact that the observations must be made instantaneously, when the platinum melts. This fact, if the observations are made visually, necessitates the employment of an unusual although simple photometric procedure. It inevitably decreases the precision of setting but does not prevent the mean of a number of observations from attaining a highly satisfactory accuracy. When the observations are made by a suitable physical method, such as that here used, the brief time available for setting is no longer a serious objection. While observation with the photo-electric cell is not at present common laboratory practice, the simplicity of the apparatus once set up and the very great accuracy attainable recommend it for use in all photometric standardizing work to which it can be applied. Granting the use of a suitable photometric procedure, the platinum black body meets the requirement of being easily and accurately measurable.

Turning now to the second point of view, the question to be answered is: How closely may the brightness of the particular platinum black body used in this experiment be identified with the beautifully simple standard proposed by Waidner and Burgess: The brightness of the black body at the melting point of platinum? Every effort has indeed been made to insure that the attempted practical realization of this scientific specification is free of special individual peculiarities due to design, dimensions or materials. We cannot, however, evade the fact that such independence of conditions special to this investigation can, in the last analysis, be proved only by experiments on other forms of black body—assuming that other forms can be set up which will equally well meet the requirements of freedom from possibility of contamination and exact temperature equivalence with the melting platinum. In default at present of such experiments the best that can be done is to examine critically the known sources of possible deviation from the theoretically ideal black body, and to marshal such evidence from other black-body investigations as is at all applicable.

Two sources of error which are readily apparent may be expected to make the measured brightness *lower* than that for a true black body. These are: Deviation of the platinum surface from true specular character, whereby the practically complete blackness,

indicated by calculation, is not attained; and irregularities of temperature around the cylinder, which will melt at the point of highest temperature, while the emission will be a function of the average temperature. The precautions taken to avoid these sources of error have already been described, and are believed to be very complete. A third source of error might raise the temperature at which the cylinders fail; this is the unequal heating of the inside and outside of the cylinder. Examination of the melted platinum often shows that the molten metal has flowed down on the inside, but never on the outside, suggesting a definitely higher temperature inside. It might accordingly be possible for the inside surface of the cylinders to be carried to the melting point and the cylinder still be held together by the outer unmelted surface. Any difference of this sort that might be serious if the radiation from the interior were being observed to get a measure of the cylinder temperature is much less serious where the metal is carried to the melting point, for two reasons: First, because immediately the inner surface starts to flow and so decreases the thickness of the wall the current density must increase and force the complete melt at once. Second, if a perceptible lag of temperature rise occurs between the inside and the outside, the diversion of energy into latent heat of fusion of the platinum will tend to hold any part attaining the melting point at that temperature until the whole body of metal reaches the same point. Whether this possible source of error is of significance depends of course on the difference of temperature which can exist between the inside and outside surfaces of the cylinder. Computations, based on the assumption that all the communicated heat is lost from the outside surface, give the temperature difference as of the order of magnitude of $\frac{1}{100}$ degree centigrade, which makes it evident that discussion of this point is largely theoretical. The outstanding recognized sources of error therefore appear to be such as will make the value here given lower than the true one. In addition to those must be included errors due to accidental defects, in particular microscopic flaws or blisters in the platinum, which would weaken the cylinder at one part and produce unequal heating. Errors from such causes would all contribute low values, emphasizing the general conclusion that the brightness value

obtained from the investigation is probably slightly lower than the true value for an ideal black body.

Before considering measurements in this temperature region on other forms of black body, there is a feature exhibited by the platinum black body, which may have a bearing on the question of the identity of its radiation with that from the ideal black body; this is the color of the light emitted. Previous observations [14] comparing the energy distribution, and the integral color, of the "four-watt" carbon lamp with the black body (*i.e.*, a carbon tube black body) have agreed in assigning to the lamp a "color temperature" of 2070° to 2080° K., at which temperature the black body has a brightness of about 70 cp./cm.^{2}. The difference in color of the black body corresponding to the difference between this temperature and the melting point of platinum (about 2035°), is approximately that between a "four-watt" and a "three and one-half watt" carbon lamp. *This is a color difference which while small should be plainly evident in the photometer field*, especially the Lummer-Brodhun field, at the illumination level used in the preliminary work, where a slit was placed directly in front of the cylinder opening. (This color difference might just be missed under the photometric observing conditions used in the final visual series of observations.) This color-temperature difference of about 35°, if real, cannot be accounted for by any uncertainties in the choice of the black-body constants. It suggests a real difference in the emission of black bodies composed of different materials, such as would be caused by some luminescent process near the melting point. If such a difference exists it would invalidate the identification of the melting platinum cylinder of black-body form with the "black body at the melting point of platinum."

Taking up now such evidence as is available from other black-body investigations, strong presumption for the substantial correctness of the result is furnished by the close agreement of the value found with that computed in the earlier part of the paper from certain selected black-body constants. The value there found (Fig. 1) gave 55.9 cp./cm.^{2}, assuming a melting point of 2037°. The value 55.4 corresponds to a melting point 2035° K., which is well within the limits of uncertainty of this point. Another piece of confirmatory evidence is furnished by the measurements of Hyde, Forsythe and Cady [18] on the brightness of a carbon

tube black-body furnace. In this investigation temperatures were measured by an optical pyrometer, working from the gold melting point; the assigned values of temperature are thus dependent on the choice of the constant C_2, for which 14,350 was used. Interpolating the value 55.4 on the curve by which they represent their results, we get a corresponding temperature of 2035° K., showing that, considering the uncertainties of the optical temperature scale and the melting point of platinum, there is substantial agreement between the results from the two different kinds of black body. As emphasized in the discussion at the beginning, however, it is not possible, so long as the various constants involved are as much in doubt as they now are, to determine whether this agreement is as close as the fraction of a degree necessary where brightness values are in question.

(*c*) *General Summary.*—I. A form of black body constructed entirely of platinum, capable of being raised to its melting point by electrical heating, has been developed and studied.

II. The brightness of this platinum black body at its melting point has been measured, and been found to be:

55.40 candle-power per square centimetre.

with an estimated accuracy of $^1/_5$ per cent. This point is proposed as a fixed point in the photometric scale, the platinum black body at its melting point constituting *a new primary standard of light.*

III. A discussion is given of the approximation of this standard to the standard proposed by Waidner and Burgess, namely "the brightness of the black body at the melting point of platinum."

(*d*) *Acknowledgments.*—The writer is indebted to a large number of people for aid given at one stage or another of this investigation. He wishes particularly to mention the assistance of Mr. E. F. Kingsbury and Mr. J. H. Chancellor in the early part of the work. He is indebted to Mr. E. C. Crittenden for the full coöperation offered at all times on the part of the Bureau of Standards. In the preparation of the chemically pure platinum his thanks are due to Dr. J. E. Harris. He wishes to acknowledge also the careful attention given to the working of the platinum by Dr. F. E. Carter, of the Baker Platinum Company. Valuable advice in the use of the string electrometer was kindly given by Dr. H. B. Williams. The success of the later part of the

study is in large degree due to the manipulative skill and careful measuring technic of Mr. A. L. Johnsrud.

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The Removal of Thorium from the Surface of a Thoriated Tungsten Filament by Positive Ion Bombardment. K. H. KINGDON and IRVING LANGMUIR. (*Phys. Rev.*, Aug., 1923.)—"Langmuir has shown that by subjecting a tungsten filament containing a small percentage of thorium oxide to suitable thermal treatment, it is possible to obtain a complete layer of thorium one atom deep over the surface of the tungsten. At a temperature of 1500° K. the electron emission from this thorium surface is about 100,000 times that from an equal area of pure tungsten. Hence removal of thorium from the surface by ion bombardment or otherwise exposes the tungsten underneath and decreases the electron emission of the filament." This paper is a study of the knocking off of the thorium layer by the impact of positive ions of various gases. For each gas a certain threshold voltage was necessary in order that the disintegration ("sputtering") of the thorium cathode should commence. For Ar, Cs, Hg and Ne ions about 50 volts sufficed, but for hydrogen 600 volts were too small. At 150 volts quite different numbers of impinging ions of the several gases were needed to remove one atom of thorium. It took about 7000 helium ions or 12 argon ions to take one thorium atom from the surface. G. F. S.

NOTES FROM THE U. S. BUREAU OF STANDARDS.*

WET-PROCESS ENAMELS FOR CAST IRON.[1]

By R. R. Danielson and H. P. Reinecker.

[ABSTRACT.]

WET-PROCESS enamelling of cast iron is becoming of importance because of its application to the enamelling of stove parts, sanitary fittings, and hardware. This paper is a report of an extended investigation of wet-process enamels for cast iron, both with and without the use of ground coat. Compositions used in dry-process enamelling have served as a basis for the work. The effect of varying methods for preparation of the frits, mill additions, and relation of composition of the enamel to such properties as adherence, texture, and opacity have been carefully studied.

Ground Coats.—Sintering of most ground-coat frits has been found desirable in order to develop best adherence of the enamels to the casting. Clay gave best results as a mill addition for the ground coats and flint and feldspar were found to be less satisfactory. Excessive additions of clay or flint produced flaking of the ground coat, while feldspar tended to develop blistering. About 15 per cent. of clay or 10 per cent. each of clay and flint are recommended as mill addition for the ground coat.

Excessive additions of any one flux were not desirable and best results were obtained when sodium, lead, and boric oxides were combined in more or less definite proportions. Sodium

Ground Coats.

Batch Composition of Frits.

Enamel.	Flint.	Borax.	Sodium Nitrate.	Soda Ash.	Red Lead.	Boric Acid.
Rg-1	69.90	36.96	6.95	.....	8.17	.....
Rg-17	69.90	36.96	6.95	.....	4.08	7.08
Rg-18	69.90	26.07	6.95	3.04	12.24	.....
Rg-25	69.90	26.07	6.95	9.88	8.17	.....
Rg-26	66.30	36.96	6.95	.....	8.17	6.37

* Communicated by the Director.

[1] Technologic Papers, No. 246, price ten cents.

oxide in excess of about 10 per cent. gave rise to blistering. Boric oxide increased the firing range of the ground coat and was preferable to lead oxide on this account, although it tended to promote crawling of ground coats high in boric oxide if they were applied somewhat heavily.

The best ground coats developed are given in the typical batch compositions on page 401.

MILL ADDITIONS.

Fifteen per cent. of clay or 10 per cent. each of clay and flint based on the weight of the frit, with such additions of water as may be necessary.

Cover Enamels.—These were smelted by the usual method for enamels. The mill additions consisted of 5 per cent. of clay, 8 per cent. of tin oxide, and 45 per cent. of water, all based on the weight of the dry frit.

For best results the cover enamel must be adapted to the ground coat in refractoriness. Boric oxide increased the firing range of the enamel, but tended to promote crawling, although this was less pronounced in the case of the more fusible composition. Boric oxide improved the capacity when substituted for such fluxes as sodium and lead oxide. Cryolite increased opacity, but additions above 10 per cent. tended to promote crawling. The following compositions are typical of those which gave satisfactory results:

White Cover Enamels.

Batch Composition of Frits.

Enamel.	Feldspar.	Flint.	Borax.	Sodium Nitrate.	Soda Ash.	Red Lead.	Zinc Oxide.	Fluorspar.	Cryolite.	Boric Acid.	Barium Carbonate.
R-1.....	38.0	12.0	24.52	6.20	3.86	16.42	6.0	5.5	5.0	....	....
R-11.....	33.0	12.0	24.52	6.20	3.86	21.60	6.0	5.5	5.0	....	....
R-14.....	38.0	12.0	10.90	6.20	7.70	21.60	6.0	5.5	5.0	....	....
R-18.....	33.0	12.0	24.52	6.20	3.86	16.42	6.0	5.5	10.0	....	....
R-28.....	40.0	....	54.5	5.95	....	10.3	5.0	5.5	....	8.85	4.5

Single-coat Colored Enamels.—These enamels are smelted in the same manner as used in the preparation of the white cover enamels. The effects of the various ingredients are very similar to those noted in the case of white cover enamels. Suitable mill

additions consist of 4 per cent. of clay, 50 per cent. of water, and the necessary amounts of color oxide to give the colors desired.

The feldspar type of enamels are apparently more satisfactory than those containing flint as the sole refractory. The following compositions are typical of those which have given very satisfactory results:

Single-coat Colored Enamels.

Batch Composition of Frits.

Enamel.	Feldspar.	Borax.	Sodium Nitrate.	Red Lead.	Soda Ash.
Ro-6.....	47.05	36.94	4.63	27.81	7.64
Ro-5.....	47.05	50.57	4.63	22.71	3.90

Single-coat White Enamels.—In the production of white single-coat enamels it is necessary to develop maximum opacity in the frit, since the addition of more than 3 or 4 per cent. of tin oxide in the mill batch caused blistering of the enamel when applied to the iron. Suitable mill additions for enamels of this type are as follows: Clay, 4 per cent.; tin oxide, 4 per cent.; and water, 45 per cent.

In no case were the results with the single-coat white enamels as satisfactory as those obtained with a white enamel applied over a ground coat, particularly in regard to the opacity. The following enamels are typical of those which gave the most satisfactory results:

Single-coat White Enamels.

Batch Composition of Frits.

Material.	Ro-16.	Ro-38.
Feldspar.....	45.05	45.05
Borax.....	50.57	39.67
Sodium nitrate.....	4.63	4.63
Red lead.....	22.71	22.71
Soda ash.....	3.85	6.87
Antimony oxide.....	2.00	2.00
Cryolite.....	0.00	4.00

Mill Batch.

Frit	100.0
Clay	4.0
Tin oxide	4.0
Water	45.0

A New Electromagnet of Great Strength. H. BOAS and T. PEDERZANI. (*Zeit. f. Phys.*, Vol. 19, p. 351.)—The weight is only 219 kilograms. A current of 25 amperes can be allowed to flow constantly without overheating, as the coils of the electromagnet, formed in flat layers with spaces between them, are cooled by an air blast furnished by a motor built into the iron circuit of the instrument. This motor takes 300 watts, while the electromagnet itself absorbs 3400 watts. A field strength of 48,000 gausses is produced over a circle 10 millimetres in diameter, when the pole pieces are 1 millimetre apart. The author is justifiably proud that he has kept the rise of temperature in his field coils, 55° C., within the limits of permissible heating set by German technicians, and, further, that by diminishing the diameter of his pole pieces, he can obtain as strong a field as du Bois got with an electromagnet weighing 65 per cent. more, and then only for a few minutes at a time. The famous magnet of Pierre Weiss, now Professor of Physics in the French University of Strasbourg, formerly at Zurich, had the disadvantage of requiring to be joined to a system of water supply in order to cool the coils. Its great weight was largely due to the oil surrounding the coils and to the water pipes in it. It took 18,000 watts, but gave a stronger field by perhaps 20 or 30 per cent. over a smaller area. G. F. S.

The Nature of the Particles of Alkali Metals that Emit Light. E. RUPP. (*Ann. d. Phys.*, Vol. 73, p. 1.)—Anode rays were generated in a tube. They passed to the cathode and some of them traversed a small canal in it and entered a chamber beyond. Here the gas pressure was not allowed to exceed .008 millimetre of mercury. The rays in this chamber passed between the plates of a charged condenser. If the particles forming the rays had electrical charges they were deflected. If they were uncharged no deflection took place. The light emitted by the particles was photographed by a spectrograph as a number of bright lines. When the electrical field of the condenser was applied some lines were deflected, while others were not. From a study of such deflections the following conclusions were reached—the primary series of lines of the alkali metals are emitted by atoms that consistently have no charge, while the first and second subsidiary series owe their origin to atoms that are positively charged before emission begins and neutral during emission. The lines of the spark spectrum of oxygen are produced by atoms that are positively charged both before and during emission.

G. F. S.

Dickhart Test for Olive Oil. H. S. MUDGE (*Cotton Oil Press*, 1923, 7, No. 8, 38) has studied the Dickhart test for olive oil which was noted in the JOURNAL OF THE FRANKLIN INSTITUTE (1923, 196, 853). Mudge finds that the test gives an entirely negative reaction with animal oils such as lard oil and neatsfoot oil. If rosemary oil be present, it may be removed by washing the sample with 80 per cent. alcohol prior to applying the test. J. S. H.

NOTES FROM THE RESEARCH LABORATORY EASTMAN KODAK COMPANY.*

MARKINGS ON MOTION PICTURE FILM PRODUCED BY DROPS OF WATER, CONDENSED WATER VAPOR, AND ABNORMAL DRYING CONDITIONS.[1]

By J. I. Crabtree and G. E. Matthews.

WATER markings are classified into the following types: (*A*) Markings produced previous to development and caused by (1) accidental splashing of water on the film, or (2) condensation of water vapor. (*B*) Markings produced after development and fixation and caused by abnormal drying conditions. The first half of the article deals with the various factors influencing the formation of the different types of markings such as the age of emulsion, temperature, nature of support, order of exposure, degree of swelling of the gelatin, etc. In general, all types of markings discussed under (*A*) are most prevalent on film that has been exposed to light before the moisture deposition occurs. The number and type of the markings were independent of the nature of the emulsion or support. Under (*B*), the formation of drying marks is prevented by hardening the film, removal of superfluous water, and a moderate, even rate of drying, whereas increased swelling of the gelatin, rapid drying at high temperatures or changes in the rate of drying encourage production of drying markings.

The second half of the paper presents a detailed classification including illustrations of fifteen kinds of moisture markings as were produced under the conditions described in the previous section. The markings may be roughly divided into three essentially different types: (1) Black spots darker than the surrounding film; (2) white spots lighter than the surrounding film; and (3) composite spots. Practical suggestions on methods of avoiding moisture markings and a short theoretical postulation of their manner of formation are given.

* Communicated by the Director.

[1] Communication No. 195 from the Research Laboratory of the Eastman Kodak Company and published in *B. J. Phot.*, Jan. 4, 1924, pp. 6, 15; *Trans. Soc. Mot. Pict. Eng.*, 1923.

MENSURATIONAL CHARACTERISTICS OF PHOTOGRAPHIC FILM.[1]

By F. E. Ross.

THE observation of Professor Cheshire and Doctor Curtis that photographic film is subject to a large and, moreover, irregular contraction during the process of development, etc., is confirmed only in the case of non-curling film. For motion picture, aerial, portrait, and process films, the contractions and irregularities are found to be considerably less than in the case of non-curling. In portrait and process film, they are only one-tenth of those found by Cheshire and Curtis, averaging but one-twentieth of 1 per cent. The changes in length and width are not the same, the length of a film showing about 30 per cent. increased effect due, probably, to strains in the roll. Film of all kinds is subject to parallel changes in length from day to day and from winter to summer, due to humidity conditions. There is no local distortion in portrait film nor in motion picture or aerial film. Local distortions of 10μ sometimes appear in non-curling film on development, which decrease and gradually disappear in a few days. The factors controlling dimensional changes appear to be humidity, together with the dissolving out in solution as well as the evaporation of low and high boiling solvents in the film base. Important in the secular or continuous gradual shrinkage usually found in motion picture film appears to be progressive changes of the film base, the effect of which on dimensional changes depends on the method and conditions of storage.

NOTE ON SIMPLICITY OF MECHANISM OF REACTION AS ONE OF THE FACTORS CONDITIONING CATALYSIS.[2]

By E. R. Bullock.

IN complex dichromate-bromide photographic solutions of the type of Ozobrome bleach, the presence of potassium ferricyanide or copper sulphate has been found practically essential. Such a substance hastens the bleaching (which otherwise may be exceed-

[1] Communication No. 192 from the Research Laboratory of the Eastman Kodak Company, published in *Phot. J.*, January, 1924, p. 37.

[2] Communication No. 186 from the Research Laboratory of the Eastman Kodak Company and published in *J. Phys. Chem.*, Feb., 1924, p. 179.

ingly slow), without itself undergoing any apparent alteration—acting therefore as a catalyzer of the reaction. An attempt to understand something of the *rationale* of the phenomenon of catalysis in these instances has led to the interesting conclusion, in accordance with a principle of simplicity, that in a certain class of reactions, namely, those between an insoluble solid and a substance in solution, it is generally possible to predict what substances (if any) will act as catalyzers.

Sodatol.—According to A. J. ADAMS (*Quarterly Bull. Agricultural Experiment Station*, Michigan Agricultural College, 1923, **6**, 52–53), sodatol is a mixture of sodium nitrate and trinitrotoluene. While it was originally intended for military purposes, it may be used as an agricultural explosive, and is more suitable for this purpose than either pure trinitrotoluene or picric acid. Sodatol is of value in blasting stumps and boulders, but can not be used for ditch blasting.
J. S. H.

Organic Titanates.—FRITZ BISCHOFF and HOMER ADKINS, of the University of Wisconsin (*Jour. Am. Chem. Soc.*, 1924, **46**, 256–259), have prepared the methyl, ethyl, isopropyl, and normal butyl esters of orthotitanic acid by the reaction of one molecular equivalent of titanium tetrachloride with four molecular equivalents of a sodium alkoxide (*e.g.*, sodium normal butylate) dissolved in the corresponding alcohol. Tetramethyl orthotitanate is a white solid, the other alkyl orthotitanates are colorless liquids. All four esters may be distilled.
J. S. H.

Whale Oil.—C. H. MILLIGAN, C. A. KNUTH, and A. S. RICHARDSON (*Jour. Am. Chem. Soc.*, 1924, **46**, 157–166) have studied the composition of whale oil by fractional distillation of the methyl esters of its fatty acids. These acids contain an even number of carbon atoms in their respective molecules; the number of carbon atoms present in the molecule ranges from 14 to 24. The approximate distribution of fatty acids in the oil is: Myristic acid (a saturated acid containing 14 carbon atoms), 4.5 per cent.; palmitic acid (a saturated acid containing 16 carbon atoms), 11.5 per cent.; palmitoleic acid (an unsaturated acid containing 16 carbon atoms), 17.0 per cent.; stearic acid (a saturated acid containing 18 carbon atoms), 2.5 per cent.; oleic and related acids containing 18 carbon atoms, 36.5 per cent., and the following unsaturated acids: Containing 20 carbon atoms, 16 per cent.; containing 22 carbon atoms, 10 per cent.; containing 24 carbon atoms, 1.5 per cent. Unsaponifiable matter is present to the extent of 0.7 per cent.
J. S. H.

Researches on the Disruptive Potential in Rarefied Gases. E. DUBOIS. (*Ann. de Phys.*, vol. 20, p. 113.)—The presence of saline contamination upon the cathode has a marked effect in lowering the disruptive potential. Even so small a quantity as a millionth of a gram of sodium chloride per square centimetre causes a considerable lowering. A vastly less important cause of variation is the occlusion of gas in the electrodes. In general this may be banished from consideration, but in the case of a palladium cathode it is not negligible by any means. The condition of the anode is without influence on the disruptive potential, the cathode only being of weight in this matter. So long as the cathodes are uncontaminated, the metal of which they are made is without effect upon the potential of disruption.

G. F. S.

The Effect of Tension on the Thermal and Electrical Conductivities of Metals. P. W. BRIDGMAN. (*Proc. Am. Acad. Arts and Sc.*, vol. 59, No. 6.)—A drawn rod of metal 6 inches long and $\frac{1}{8}$ inch in diameter was held in a vertical line. Copper grips grasped it at the two ends. At its middle a heating coil was wound. By copper-constantin thermo-elements the difference of temperature between two points 6.5 mm. apart was measured at two positions symmetrically located, one above, the other below the heating coil. From the heat input, these temperature differences and the constants of the apparatus the thermal conductivity can be computed. In measuring the electrical conductivity "current was passed lengthwise through the specimen, and the potential difference determined between two needle points pressed into contact with the specimen by a simple spring arrangement." When the metal cylinder was stretched by applying a downward force at its lower end both the thermal and the electrical conductivities were discovered to be less in the cases of Al, Cu, Fe, Pd, Pt and Ag. In Ni both increased. Al manifested the largest proportional changes per kg./cm.^{2} load, 6 and 4 millionths of the initial values for the thermal and the electrical conductivities, respectively. The reductions in the two conductivities are not the same for any metal used. Hence the ratio of the conductivities does not remain constant as it should were the Wiedmann-Franz relation strictly correct. In Ni the rate of increase for electrical conductivity is about six times as great as for the thermal quantity.

As to his selection of metals the experimenter comments: "I used most of the cubic metals that can be obtained in a state of purity and homogeneity, and which are at the same time of sufficient mechanical strength to stand the handling and the tension; there are not as many of these metals as might be supposed. Non-cubic metals I did not think it worth while to trouble with, because I have recently been finding that there are very large differences in different directions. Such metals should be examined in the state of single crystal grains."

G. F. S.

NOTES FROM THE U. S. BUREAU OF CHEMISTRY.*

AN INVESTIGATION OF OYSTER SPOILAGE.[1]

By Albert C. Hunter and Bernard A. Linden.

[ABSTRACT.]

THE spoilage of shucked oysters held at various temperatures, with and without ice, is correlated with the hydrogen-ion concentration of the oyster liquor.

Hydrogen-ion concentration values were determined for good, stale, and sour oysters obtained from the Potomac River, shucked, washed, and stored under different conditions.

Total aërobic counts of bacteria in the oyster liquor fail to correlate with the physical condition and hydrogen-ion concentration of the decomposing oysters.

Yeast counts, the numbers of lactose fermenting organisms, and the bacterial counts from the oyster meats are too variable to be of significance in determining the spoilage of oysters.

A determination of the numbers of certain types or species of bacteria responsible for the spoilage, and as yet unidentified, will probably be of assistance in judging the quality of shucked oysters.

Autolysis plays a part in softening or digesting the oysters.

The method of shucking and washing appears to have little effect on the keeping quality of the oysters, but a liberal use of ice during storage is necessary to deliver shucked oysters to distant markets in an edible condition.

THE INFLUENCE OF VARIATION IN CONCENTRATION ON THE ABSORPTION SPECTRA OF DYE SOLUTIONS.[2]

By Walter C. Holmes.

[ABSTRACT.]

IN a preliminary study of the variation in absorption of over one hundred dyes resulting from the variation of the concentration of their aqueous solutions over a wide range, a radical type of modification in absorption was established for a large number of dyes. This is taken as evidence that the dyes in question are present in aqueous solution in a state of tautomerism between two constitutional forms, with the equilibrium depending upon the concentration of the solution.

* Communicated by the Chief of the Bureau.

[1] Published in *Am. Food J.*, **18** (Nov., 1923) : 538.

[2] Published in *Ind. Eng. Chem.*, **16** (Jan., 1924) : 35.

High-pressure Reheating Turbine.—The first practical application in this country of this type of prime mover is the installation at the Philo, Ohio, plant of the Ohio Power Company, a subsidiary of the American Gas and Electric Company, the first unit of which was recently shipped by the General Electric Company. High-tension transmission lines are to be tied in on one side of this station as far east as Pittsburgh and will, no doubt, ultimately be on the west as far as Chicago.

The unit just shipped is the first of six to be ultimately installed. Each is a 35,000-k.w., 1800-R.P.M., 11,000-volt, 60-cycle, single-cylinder turbine-generator set, arranged for reheating and multi-stage extraction. This will operate at 600 pounds steam pressure and 725° F. steam temperature. The entire project is a practical step in the super-power conception now being agitated and is a definite move towards the conservation of natural resources. H. L.

The Determination of the Time between Excitation and Emission for Certain Fluorescent Solids. P. F. GOTTLING. (*Phys. Rev.*, Dec., 1923.)—"It has been shown recently that some fluorescent substances remain dark during a definite period of excitation, in other words, that the exciting energy was imprisoned for a short but definite and measurable interval of time within the fluorescent substance. In the case of mercury vapor, the period intervening between the beginning of the process and the emission of the light was found to be about 1/15,000 second. Similar observations made with barium-platino-cyanide indicated that not only was this interval of time less than 1/400,000 second, but, furthermore that the total duration of the phosphorescence, after the exciting incident energy ceased to fall upon the fluorescent substance, was also very short."

For the measurement of the exceedingly brief periods involved, a modification of the method of Abraham and Lemoine was used. For the two substances barium-platino-cyanide and a solution of rhodamine in acetone, acetic acid and glycerine, the intervals between the occurrence of the spark and the beginning in the two substances of the fluorescence caused by the light of the spark were, respectively, equal to the time light requires to travel 6362 and 632 cm. in air, or to 2.12×10^{-7} and 2.11×10^{-8} seconds. G. F. S.

Mica.—DOUGLAS B. STERRETT has published a comprehensive monograph on the mica deposits of the United States (*U. S. Geol. Survey Bull.*, 740, 354 pp., 29 plates, 96 figures, Washington, 1923). Material for this treatise has been collected at intervals during a period of twenty years. The geology and mineralogy of mica are discussed; and its mining, preparation for market, and uses are described. An account is given of the domestic mines by states and counties. Mica has been mined in the United States since 1803. Mines occur in twenty-six states. The Pennsylvania deposits are in Berks, Lehigh, Chester, and Delaware Counties. A bibliography of 57 references and 101 references as footnotes increase the value of the bulletin. J. S. H.

NOTES FROM THE U. S. BUREAU OF MINES.*

QUALITY OF MOTOR GASOLINE.

By N. F. LeJeune, I. H. Nelson, and L. P. Calkins.

RESULTS of the ninth semi-annual survey covering gasoline sold in American cities indicate that the average motor gasoline being marketed this winter apparently possesses slightly better engine-starting qualities than that tested in former winter surveys.

In certain districts petroleum refiners are obtaining a much better fractionation of the lighter products, thus permitting an increased yield of gasoline without appreciably affecting the quality. About two years ago the 90 per cent. distillation point in Federal specifications was raised in order to permit more gasoline to be obtained from a given quantity of crude oil, while still maintaining a motor fuel of satisfactory quality. At the time this change was made the end point in the distillation was allowed to remain as it had previously stood, and a question arose as to whether refiners would be able to take advantage of the increase in the 90 per cent. point and still maintain the old end point. The results of the present survey indicate that this is being done successfully in certain districts.

With the exception of these indicated changes, the Bureau of Mines found that the winter grade of gasoline now being marketed is not materially changed from that of recent years. Details of the ninth semi-annual motor gasoline survey are given in Serial 2577.

EFFECTIVE TEMPERATURES FOR STILL-AIR CONDITIONS AND THEIR APPLICATION TO MINING.

By F. C. Houghton, C. P. Yaglaglou, and R. R. Sayers.

ATMOSPHERIC conditions relative to high temperature and humidity are of great importance in mining, as well as in such allied fields as railroad and vehicular tunnels. An extended investigation into the effect of these factors on men has been in progress for some time, both in the field under actual working conditions, and at the Pittsburgh experiment station of the bureau in coöperation with the American Society of Heating and Venti-

* Communicated by the Director.

lating Engineers and the U. S. Public Health Service. For several years one of the writers (Doctor Sayers) and D. Harrington, supervising mining engineer of the bureau, have been studying the effects of hot humid atmospheres in metal mines on the comfort and efficiency of the workers. Some of the reactions experienced in still and in moving saturated mine air at temperatures of 90 to 100° F., with men at rest, are presented in an earlier paper.

More recently, experiments were made under carefully controlled conditions to determine the relative feeling of warmth experienced by men in atmospheres of various degrees of temperature and humidity, by the research laboratory of the American Society of Heating and Ventilating Engineers, at the Pittsburgh station of the bureau, in special psychrometric chambers built for this purpose. Investigations of the effects of moving air are being continued.

A comfort chart was plotted which shows the relative degree of comfort to be expected at different combinations of temperatures and humidities in still air.

The conclusions drawn from the present tests are as follows:

(1) Comfort, as determined by both sense and physiological reactions, depends solely upon effective temperature. At 32°, the effective-temperature line coincides with the dry-bulb temperature line; hence, in this particular case dry-bulb temperature is the only factor in determining comfort. (2) In the comfort zone, comfort depends equally upon wet and dry-bulb temperatures. (3) At about 132° the effective temperature coincides with the wet-bulb temperature, and for this case the wet-bulb temperature is the only factor. (4) Below 32° the effect of humidity is reversed —that is, the lower the humidity the greater the feeling of warmth. Further details are shown in Serial 2563.

THE KATA THERMOMETER—ITS VALUE AND DEFECTS.

By W. J. McConnell and C. P. Yaglaglou.

THE kata thermometer is frequently used in studies of mine ventilation conditions with regard to comfort and health of miners. It has been used in the studies being conducted along this line by the Bureau of Mines, on the effects of high temperatures and humidities of air in mine workings on the comfort conditions, as well as in studies of atmospheric conditions in mine, railway and vehicular tunnels.

A recent study of the fundamentals of the kata indicates that the kata thermometer, while a useful instrument, stands in need of more experimentation. It is possible by partly covering the bulb with a wet cotton glove to obtain a satisfactory approach to the conditions of the human body. Arrangements may be made by means of expanding and contracting metal strips automatically to vary the wet and dry surface of the bulb, according to the temperature conditions of the air, so as to approach the various degrees of wetness of the human body at various atmospheric conditions.

Perhaps nothing constitutes better information than that obtained in the field with human beings as subjects. The research laboratory of the American Society of Heating and Ventilating Engineers in coöperation with the Pittsburgh experiment station of the Bureau of Mines established a composite index of comfort for still-air conditions, called " Effective Temperature Index."

It was found that several combinations of high temperatures and low humidities were sensibly and physiologically equivalent to other combinations of comparatively lower temperature and higher humidity. Hence while a person will feel equally hot or cold under all equivalent conditions, kata thermometer observations will vary radically under the same conditions, depending on the dry-bulb temperature alone when dry kata observations are made and on the wet-bulb temperature when wet kata observations are made. Furthermore, the relative importance of the wet-bulb and dry-bulb temperature, as affecting human comfort, was found to vary with temperature. At 32° the dry-bulb temperature was found to be the only index of comfort, while at about 132° the wet-bulb temperature constituted such an index, regardless of dry bulb. At 67° saturation temperature the two temperatures were of equal importance.

It follows, therefore, that at 32° in still-air conditions the rate of heat loss of the dry kata corresponds to that of the human body, and at 132° the rate of heat loss of the wet kata corresponds to that of the human body. To take care of the changing predominancy of the two temperatures, the arrangement of the expanding members proposed above should be such as to free completely the kata bulb of the cotton glove at 32° and cover it completely at 132°, water being supplied to the lower end of the cotton glove and carried up automatically by capillary action.

The effects of air motion on human comfort are being studied at present by the research laboratory of the A.S.H.V.E. and the Bureau of Mines, at Pittsburgh, Pa. On the completion of the work a certain relation may be found between kata cooling powers and effective temperatures which will probably greatly increase the usefulness of the kata thermometer. Further details are presented in Serial 2565 of the bureau.

METHODS OF TESTING DETONATORS.

By C. A. Taylor and C. E. Munroe.

IN blasting the explosion of the high explosives charge is initiated by means of a detonator. Consequently the successful operation of the whole system is dependent upon the suitability and efficiency of the detonator used. A considerable number of methods for testing the efficiency of detonators have been devised. In 1910, W. O. Snelling, then explosives chemist of the bureau, devised the vessel known as the Bureau of Mines sand-test bomb. Later, Storm and Cope made very extended tests of this method. Their paper contains a detailed, illustrated description of sand-test bomb No. 1, with precise specifications of the sand employed and a minute account of the technic followed.

The results of more recent work by the writers emphasize the value of the sand test as a satisfactory method for estimating the strength and efficiency of industrial detonators and for fixing standards by which different types of detonators may be judged. As detonators of new compositions such as lead nitride or trinitroresorcinate are put on the market, a series of tests on each different brand can easily establish limits wherein each new type may be classified. Where detonators are to be tested in ordinary lots, or in commerce where a general classification rather than a precise chemical determination is desired, the sand-test method affords the simplest means by which this can be carried out. Its simplicity, and the ease with which it may be operated, mark its superiority and assure its practicability. Its efficiency may be somewhat impaired by the occurrence of variables of different natures; but this may be remedied to a great extent by standardization of materials and conditions of operation. A fuller presentation of these recent tests will be found in Serial 2558 of the bureau.

THE FRANKLIN INSTITUTE.

MINUTES OF THE ANNUAL MEETING OF THE BOARD OF MANAGERS FOR ORGANIZATION.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, January 23, 1924.

The Meeting of the Board of Managers for Organization was held this day at 3.30 o'clock P.M.

Members Present: Mr. W. C. L. Eglin, President, in the Chair; Messrs. Charles E. Bonine, Walton Forstall, Benjamin Franklin, W. H. Fulweiler, Clarence A. Hall, Charles A. Hexamer, Edward V. McCaffrey, James S. Rogers, Haseltine Smith, C. C. Tutwiler, William Chattin Wetherill, Doctors Walton Clark, George A. Hoadley, Harry F. Keller and R. B. Owens. Total, 17.

Members Excused: Messrs. Francis T. Chambers, G. H. Clamer, Theobald Clark, Charles Day, Alfred C. Harrison, Nathan Hayward, Henry Howson, Robert W. Lesley, Marshall S. Morgan, Lawrence T. Paul, J. T. Wallis and Dr. George D. Rosengarten. Total, 13.

The President said:

(1) The first business in order is the election of a Secretary. (On motion of Mr. Benjamin Franklin, seconded by Mr. Hall, Doctor Owens was unanimously reëlected Secretary of the Institute.)

The second business in order is the appointment of an Actuary. I have much pleasure in advising you that I have reappointed Mr. T. R. Parrish, Actuary, and I ask for your approval. (On motion of Doctor Clark, duly seconded, this appointment was approved.)

The third business in order is the election of members of the Committee on Science and the Arts. Doctor Alleman proposed the following gentlemen for members of the Committee, and on motion of Mr. Forstall they were duly elected:

(Reëlected for three years.)

Roger F. Brunel,
George S. Crampton,
George S. Cullen,
Owen B. Evans,
George A. Hoadley,
Harry F. Keller,
Leo Loeb,
K. G. Mackenzie,
Edward V. McCaffrey,
Stephen Y. Noyes,
Frank H. Rogers,
John M. Rusby,
William O. Sawtelle,
John C. Trautwine, 3rd,
C. C. Tutwiler,
John A. Vogleson,
John M. Weiss,
William C. Wetherill.

(Elected for three years.)

Edward W. Smith,
Howard W. Welles.

The fourth business in order is the appointment of Board Committees. I appoint the following Committees and ask for your approval:

ELECTIONS AND RESIGNATIONS.

Robert W. Lesley, Chairman;
Gellert Alleman,
Charles E. Bonine,
Clarence A. Hall,
Haseltine Smith.

ENDOWMENT.

G. H. Clamer, Chairman;
Benjamin Franklin,
Alfred C. Harrison,
George D. Rosengarten,
C. C. Tutwiler.

EXECUTIVE.

E. H. Sanborn, Chairman;
Charles Day,
Walton Forstall,
Alfred C. Harrison,
J. T. Wallis.

EXHIBITIONS.

Benjamin Franklin, Chairman;
Francis T. Chambers,
Theobald F. Clark,
Charles A. Hexamer,
Robert W. Lesley.

INSTRUCTION.

Lawrence T. Paul, Chairman;
Gellert Alleman,
George A. Hoadley,
Harry F. Keller,
E. H. Sanborn.

SECTIONAL ARRANGEMENTS.

Harry F. Keller, Chairman;
Gellert Alleman,
Clarence A. Hall,
Robert W. Lesley,
C. C. Tutwiler.

PUBLICATIONS.

George D. Rosengarten, Chairman;
Gellert Alleman,
George A. Hoadley,
Harry F. Keller,
E. H. Sanborn.

STOCKS AND FINANCE.

Walton Forstall, Chairman;
Benjamin Franklin,
Marshall S. Morgan,
Alfred C. Harrison,
James S. Rogers.

FRANKLIN FUND AND BUILDING COMMITTEE.

Henry Howson, Chairman.

Charles E. Bonine,
Marshall S. Morgan, Treasurer;
Alfred C. Harrison,
E. H. Sanborn.

(On motion of Doctor Hoadley, seconded by Mr. Rogers, these appointments were approved.)

(2) That Dr. Walton Clark had appointed a special Committee consisting of E. H. Sanborn, Chairman; Charles Day, Walton Forstall, Benjamin Franklin, Harry F. Keller and James S. Rogers, to revise the By-Laws of the Institute and asked confirmation of these appointments. (On motion, duly seconded, these appointments were confirmed.)

(3) That the Bartol Research Committee is a continuing one, charged with carrying into effect the intent and purpose of the Bartol bequest.

(4) That the Institute at a meeting held on Wednesday, November 17,

1920, had requested the President to appoint, with the approval of the Board, a Committee to be known as the Bartol Research Foundation Committee, to consist of five members with the President and Secretary as members ex-officio.

(5) That while the retiring President, Doctor Clark, wished to be excused from service on other committees, it would be agreeable to him to serve on the Bartol Research Foundation Committee and that this could be arranged by a motion at the next regular meeting of the Institute.

(6) That before briefly outlining certain policies which he felt would be constructive and which might be adopted with profit, he wished first to thank the Board and members of the Institute for the great honour which had been done him in electing him President of The Franklin Institute. And continued:

"As I see it, the several present activities of the Institute, its Lectures, its Publications, its Library work, the work of its Science and Arts Committee and its Evening Classes have been developed under the guidance of Doctor Clark to an efficiency or effectiveness of one hundred per cent. Particularly have the contacts of the Institute increased in number and value as well as the number of those who daily take advantage of the service which the Institute renders, both in this country and elsewhere. However, I see a great deal of further good work ahead of us. We believe our methods, the result of long experience, are correct and are rightly applicable to the increasingly great number of opportunities everywhere arising to render service in the field of science, both pure and applied.

"Our present membership is excellent as to quality, but I feel it should be greatly increased. However, I do not anticipate a time when membership dues will add sufficiently to the funds of the Institute to allow us to carry on all of the great work we are capable of doing. It would not, I think, be the part of wisdom to increase the dues of our several present classes of membership. In this matter we must be guided in a measure by the practice of the more notable national scientific and technical organizations. Perhaps doubling or trebling our present membership in its several classes, which I think can be done, would mean an increased revenue sufficient to meet the present annual deficits, but I do not think it is possible to enlarge our present activities and develop the research end of the Institute as it should be developed even with the Bartol bequest without adding very greatly to our endowment funds.

"As to increasing our membership, I think it is the intention of the Committee on the Revision of our By-Laws to recommend that Permanent Membership in the Institute be increased to five thousand ($5000) dollars and that such permanent memberships be made available to firms, corporations and organizations as well as to individuals, also that we have an additional class of members to be known as Contributing Members, which may consist likewise of firms, corporations and associations paying an annual fee of three hundred ($300) dollars. The feeling is that organizations like any one of the great electrical, steel, transportation or other industrial companies would take out a Permanent or Contributing Membership and nominate—probably largely from their scientific and technical staffs—ten to twenty men who, should their qualifications be deemed sufficient by your Committee on Admissions, might be elected to membership by you. I believe through Permanent and Contributing Members we can increase our revenue this year to balance our budget, but

as I said before, we must look to endowment if we are to greatly increase our activities and extend our research facilities.

"The Institute's original purpose, to disseminate useful scientific and technical information and encourage research, has not changed within the century, though that branch of applied science which one hundred years ago was called the mechanic arts, has now come to be known as engineering, and the chairs of natural philosophy of some generations ago have become professorships in physics or chemistry.

"While realizing that all the present activities of the Institute must not only be continued—indeed they must be further developed, if possible—I feel that the greatest single field of future usefulness is physical research, for in this field particularly we shall have not only the moral support, but the generous coöperation and aid of American industries, many of which owe their inception to the early work of this splendid Institution of which we are all proud to be members.

"Another matter. Not as affording revenue, indeed perhaps on the contrary, I think we might elect certain of the men doing physical research in America and elsewhere, as Fellows of the Institute. Fellows would, of course, be properly qualified and they would be allowed the research and other facilities of the Institute under suitable regulations drafted by the Bartol Research Committee. These fellowships I have in mind would be Institute Fellowships and would be in addition to the Bartol Research Fellowships, to which we are already committed.

"From what has been said, I think you will agree that it is not only the collective duty of this Board, but the individual duty of the Institute's officers, managers and members to set about at once to provide the necessary funds for the great expansion of the Institute's work awaiting us as the result of the achievements and present state of high preparedness effected in the last decade and a half. As I have before intimated, I believe the great future work of the Institute lies in putting physical research in this country on the highest possible plane. This city is a particularly suitable place in which to conduct physical research and this Institute is a tempered instrument ready to the hands of those far-seeing persons who realize that industrial civilization presents problems so numerous and difficult that an attempt at solution without a simultaneous attempt to extend our knowledge of fundamental physical processes would be but to lead a forlorn hope. We must resolve ourselves each into a committee of one to see that no available and proper source of funds for our work is overlooked. To recapitulate we need funds—large funds—for the extension of our present activities and for the inauguration of new ones, particularly in the field of research. We need buildings, a whole group of new buildings, on the splendid site we own on the Parkway; a main building to contain lecture rooms and offices, and above all, to properly house the unique collection of priceless scientific and technical literature gathered together here throughout the past one hundred years. We need new laboratories and we need an operative scientific and technical museum where experiments illustrative of the most recent results of physical research may be seen and where the latest devices looking to the economizing of energy, physical and mental, and of the effecting of the control of the elements of time and space may be exhibited.

"I feel that our new buildings should illustrate to the fullest degree that form of artistic expression, namely architecture, for which America in recent years has become famous. With your help and the help of the many other good friends of The Franklin Institute scattered far and wide, I believe we can do in the next few years what I have briefly outlined.

"Again, gentlemen, I thank you."

Adjourned.

T. R. PARRISH,
Actuary.

(Proceedings of the Stated Meeting held Wednesday, February 20, 1924.)

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, February 20, 1924.

MR. W. C. L. EGLIN, President, *in the Chair.*

The Board of Managers submitted their report. The report recorded the election of the following Resident Members:

Donald Archer Nightingale, Esq.,
Chemist,
Ambler, Pennsylvania.

Herbert J. Tily, Mus. Doc.,
Vice-president, General Manager and Musical Director,
Strawbridge and Clothier,
Philadelphia, Pennsylvania.

Colonel Joseph J. Vogdes, C.E.,
Brann and Stuart Company,
Philadelphia, Pennsylvania.

Sidney F. Tyler, Esq., Banker,
Land Title Building,
Philadelphia, Pennsylvania.

James Lloyd Weatherwax, A.M.,
Physicist,
Philadelphia, Pennsylvania.

and the following Non-resident Members:

Samuel L. Jodidi, Ph.D., Chemist,
U. S. Department of Agriculture,
Washington, D. C.

John Johnston, D.Sc., Chemist,
Director, Sterling Laboratory, Yale University,
New Haven, Connecticut.

S. Sonoda, Esq.,
Kaida 15,
Inari, near Kyoto, Japan.

and additions to the Library by gift, 18 volumes, 16 pamphlets and by purchase, 18 volumes.

The Chairman recognized Mr. Francis T. Chambers, who offered the following resolutions:

Resolved, That Article III—Members, By-Laws of the Institute—be amended by inserting after Section 3, Section 4. There shall be in addition to present classes of membership a class of membership known as Contributing Membership, which shall consist of firms, corporations, associations, or individuals, who shall pay an annual sum of $300, with the privilege of nominating to the Board of Managers members of their staffs or others engaged or interested in scientific pursuits or in the application of physical discovery in the industries, in number to be determined by the Board of Managers.

Resolved Further, That present sections of Article III—Members, numbered 4 to 12 inclusive, be numbered 5 to 13 inclusive.

On motion, duly seconded, action on this resolution was, in accordance with Section XVII of the By-Laws, deferred until the next Stated Meeting.

The Chairman again recognized Mr. Chambers. Mr. Chambers said that the Board of Managers had, at their meeting held on Wednesday, February 13, 1924, adopted the following resolution:

Resolved, That the Board of Managers of The Franklin Institute recommend to the Institute the election to Honorary Membership of Charles A. Coffin, Esquire, "in recognition of vision, courage and constructive ability, signally contributing to the upbuilding of electrical and other industries—basic factors in modern civilization."

On motion, duly seconded, Mr. Coffin was unanimously elected to Honorary Membership in the Institute.

The Chairman again recognized Mr. Chambers, who said that the Board of Managers, at their meeting held on Wednesday, February 13, 1924, had also adopted the following resolution:

Resolved, That the Board of Managers recommend to the Institute at its next meeting to be held on Wednesday, February 20, 1924, the election of Dr. Ralph Modjeski, Chief Engineer and Chairman of the Board of Engineers of the Delaware River Bridge Joint Commission, Honorary Member of The Franklin Institute and Franklin Medallist, to membership on the Board of Managers.

On motion, duly seconded, Doctor Modjeski was unanimously elected to membership on the Board of Managers.

The President again recognized Mr. Chambers, who proposed the following preambles and resolutions, which were duly seconded and adopted unanimously:

Whereas, The President of the Institute informed the Board of Managers at their Organization Meeting held on Wednesday, January 23, 1924, that it would be agreeable to Dr. Walton Clark, who lately retired from the Presidency of the Institute, to serve on the Bartol Research Foundation Committee, and

Whereas, The Bartol Research Foundation Committee desires to avail itself of the counsel and advice of Doctor Clark, be it

Resolved, That for the purposes of availing itself of the help and counsel of Doctor Clark, the Bartol Research Foundation Committee be increased one in number, and

Resolved Further, That the President of the Institute be requested to appoint with the approval of the Board of Managers Dr. Walton Clark a member of the Bartol Research Foundation Committee.

The Chairman then introduced H. L. Cooke, M.A., Professor of Physics, Princeton University, Princeton, New Jersey, who presented the paper of the evening on "Aeroplane Surveying."

The speaker discussed the nature of the information obtainable from air photographs, described the two types of photographs and outlined the relative advantages and limitations of each. He referred to the advantages of wide angle views and said that the determination of aeroplane position is the fundamental problem of topographical survey from the air. He also discussed the nature and complexity of the problem, the three general methods of solution and the relative advantages and disadvantages of each. The speaker also described the methods of horizontalizing the views in map drawing, discussed stereoscopic methods and anaglyphic methods; he reviewed the present state of development and probable lines of progress, pointing out the ultimate limitations of air surveying.

The Chairman felt that Professor Cooke had rendered a very great service to Institute members and to others in setting forth clearly the nature of the problem of making a satisfactory topographical survey from the air and in indicating with equal clearness an elegant method of solution. On motion a rising vote of thanks was extended to the speaker and the meeting adjourned.

R. B. OWENS, *Secretary.*

COMMITTEE ON SCIENCE AND THE ARTS.

(Abstract of Proceedings of Stated Meeting held Wednesday, February 6, 1924.)

HALL OF THE INSTITUTE,
PHILADELPHIA, February 6, 1924.

MR. GEORGE H. BENZON, JR., *in the Chair.*

The following report was presented for final action:

No. 2799: Gear-tooth Grinding Machine.

The following report was presented for first reading:

No. 2813: Bahnson Humidifier.

R. B. OWENS, *Secretary.*

MEMBERSHIP NOTES.

ELECTIONS TO MEMBERSHIP.

(Stated Meeting, Board of Managers, February 13, 1924.)

RESIDENT MEMBERS.

DONALD ARCHER NIGHTINGALE, Esquire, Chemist, Ambler, Pennsylvania.

DR. HERBERT J. TILY, Vice-president, General Manager and Musical Director, Strawbridge and Clothier, Philadelphia, Pennsylvania.

SIDNEY F. TYLER, Esquire, Banker, Land Title Building, Philadelphia, Pennsylvania.

COLONEL JOSEPH J. VOGDES, Brann and Stuart Company, Philadelphia, Pennsylvania.

JAMES LLOYD WEATHERWAX, A.M., Physicist, 932 North Sixty-fifth Street, Philadelphia, Pennsylvania.

NON-RESIDENT MEMBERS.

DR. SAMUEL L. JODIDI, Chemist, United States Department of Agriculture, Washington, District of Columbia.

PROFESSOR JOHN JOHNSTON, Director, Sterling Laboratory, Yale University, New Haven, Connecticut.

MR. S. SONODA, Kaida 15, Inari, near Kyoto, Japan.

CHANGES OF ADDRESS.

MR. STERLING H. BUNNELL, 950 Madison Avenue, New York City, New York.

MR. E. M. FITZ, care of The Pennsylvania Railroad System, Syndicate Trust Building, St. Louis, Missouri.

MR. RICHARD P. HARVEY, R. F. D. No. 10, Box No. 565, Los Angeles, California.

MR. GEORGE E. KINNIER, 1109 North Maple Street, Bethlehem, Pennsylvania.

MR. EUGENE MERZ, The Heller and Merz Company, 338 Wilson Avenue, Newark, New Jersey.

MR. D. BRUCE MORGAN, Hotel St. Paul, Sixtieth Street and Ninth Avenue, New York City, New York.

MR. GEORGE A. ORROK, 124 East Fifteenth Street, New York City, New York.

MR. J. E. SYMONS, 179 Halsted Street, East Orange, New Jersey.

MR. ROBERT L. WOOD, Erdenheim Post Office, Montgomery County, Pennsylvania.

NECROLOGY.

Mr. R. H. Day, 505 West Clapier Street, Philadelphia, Pennsylvania.

Mr. Charles E. Foster, 120 St. George's Road, Ardmore, Pennsylvania.

Dr. Harry F. Keller, 103 West Upsal Street, Germantown, Philadelphia, Pennsylvania.

Mr. Alfred Moore, 618 North American Building, Philadelphia, Pennsylvania.

Mr. Edgar Richards, 60 Ayrault Street, Newport, Rhode Island.

Mr. S. P. Sharples, 22 Concord Avenue, Cambridge Massachusetts.

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BOOK REVIEWS.

MARINE PRODUCTS OF COMMERCE. Their acquisition, handling, biological aspects, and the science and technology of their preparation and preservation. By Donald K. Tressler, Ph.D., U. S. Bureau of Fisheries; Fellow of the Mellon Institute. Assisted by seventeen collaborators. 762 pages, including contents and index, 257 illustrations, 8vo. New York, The Chemical Catalog Company, 1923. Price, $9 net.

"Thalassa, thalassa," "the sea, the sea," was the joyous exclamation of the vanguard of the weary army of Xenophon, when they beheld the shore of the Euxine, and knew that they would soon be embarked in the "hollow ships" and homeward bound. Most profound has been the influence of the sea upon the destinies of nations, and to-day it is still the most potent influence. The control of the sea means the control of the world. Byron said that man marks the earth with ruin, but his control stops with the ocean shore. This may never have been strictly true, but it is not true to-day, for airplane and submarine have brought the widest and deepest ocean within human subjection. In proportion to the traffic, the dangers of the sea are now comparatively slight, though in the phases which it is the purpose of the work in hand to set forth the risks are still serious and the toll of lives taken still large. The course of events in the American colonies was governed largely by the coast contour. The fact that from the region of Canada to the Gulf of Mexico, the coast line is indented with many harbors, several of them estuaries of large rivers navigable for many miles by deep-draft vessels, had a powerful influence in determining the establishment of many separate centres of colonization. The successive waves of emigration from Europe, comprising peoples of different, and even strongly antagonistic, opinions and methods, were enabled to find an entrance to the land independent of each other. Had the coast line been like that of the western boundary of the United States, such distribution would have been impossible. From the Columbia River, about latitude 46 N., to the boundary of Lower California, a distance of over a thousand miles, the shore is unbroken by a navigable stream, and there are but three good harbors.

It is an interesting point about the sea, that, except certain near-shore areas, it is free from personal control. In the open ocean there is no "water-lord." Of course, ships passing to and fro may at any time be subject to control in the interest of a nation sufficiently powerful to institute it. During the late war, the control of the sea was at all time under Great Britain. The commerce of its enemies, except as to a few commercial submarines, was swept from the ocean, but the control was, indeed, maintained at considerable cost, owing to the activity of the enemies' raiders and submarines. Mahan has shown the great influence of the sea power in history. Its importance has been evident from the days of the thousand ships that went to shores of Troy to the present hour.

The book in hand deals with the sea as exploited by the arts of peace. It is designed as an aid to the practical man, and also to give information to the scientist. It contains an enormous amount of information, the imparting of which is very much aided by the numerous and excellent illustrations. The data are drawn from many sources, and the large list of collaborators has rendered possible the production of an encyclopedia of the products of the sea. In addition to the formally appointed collaborators, the author acknowledges obligations to a large number of persons named, also many not named, who have aided in furnishing information. The work is intended to bring the practical man in contact with the scientific features of the exploitation of the sea, and furnish the scientist with practical information as to the nature of the products of the sea and the methods of obtaining, preparing and marketing them. The scope is not limited to food products, but includes all commercial forms, special attention being given to American operations and methods. Among the topics that have been specially developed is the manufacture of salt from sea-water. In this section, the composition of water from oceans and seas in many parts of the world is given. Twenty-four separate analyses are given, Clarke's tabulations being the basis of the compilation. The data relate only to the inorganic constituents present in appreciable amount. The figures are in percentages. A slight typographic error has occurred in column E (Baltic water), the figures for sodium being printed 3.047 instead of 30.47. It is commendable that the data are given in the individual ions and not in any assumed arrangement, as was the custom with the older analysts. It is also commendable that the absurd method of the earlier German chemists of carrying out the calculations to four or five decimal places has disappeared from tabulations of water analysis.

It is impossible to set forth in the space available a full account of the contents of this book. It is, as already said, an encyclopedia of the sea. The introduction notes that the water of earth covers three-fourths of its surface, and though it appears to be a waste, it is teeming with plant and animal life. Its commercial and meteorologic importances are, of course, not considered. The largest animals and the tallest plants grow in the ocean. The sea-serpent is not mentioned; the octopus is noticed briefly, and cuttle-fishes generally are relegated to a minor paragraph. This is entirely justifiable, but the attention of the author may be called to the paper by Madoka Sasaki, on the cuttle-fish largely eaten in Japan, *Ommastrephes sloani pacificus*, published in the

Transactions of the Wagner Free Institute of Science of Philadelphia (vol. 9, part 2).

There are many interesting chapters in the book, but a special interest attaches to the discussion of the kelp industry of the Pacific coast. The shortage in potassium salts brought about by the blockade of Germany, compelled the United States to seek local sources, and one of the most promising was the giant algæ of the Pacific. A large industry grew up. Immense beds of these, generally termed "kelp," are spread over the ocean from Mexico to Alaska. Several species are represented. Much research work was devoted to determining the best method of extracting the potassium salts. The history of this work and the methods of treating the material are given. The Hercules Powder Company operated an extensive plant for fermenting kelp to obtain calcium acetate from which to prepare acetone and acetic acid, a serious shortage of which occurred in consequence of the war conditions. Stenhouse in 1851 had shown that calcium acetate could be obtained from kelp. The Hercules Company's plant cost about $2,000,000, and was capable of handling 24,000 tons of kelp a month. The plant, however, shut down promptly after the armistice. The future of the kelp industry is stated to be not very promising as all plants are now shut down, though Turrentine, who has given special attention to the subject, believes that the kelp industry can be maintained at a profit if all by-products are utilized. During the war it was the second in importance in the supply of potassium salts to this country. The cost of harvesting and the large amount of water in the fresh material are the principal difficulties.

Outside of all the merits of the book in the matters of thoroughness and comprehensiveness, the details of the arrangement deserve high commendation. An enormous collection of references has been made. In these the year as well as the volume is given. An extensive table of contents and a full index add to the convenience. The publisher has made the book in form commensurate with its merits. Printed on excellent paper in clear type, with more than twelve score illustrations, many tables and diagrams, the work will stand in the front rank of the many commendable volumes issued by the Chemical Catalog Company, and will be highly acceptable to those concerned with the many different phases of sea industries. HENRY LEFFMANN.

L'ATOMISTIQUE. By Bernard Bavink, translated into French by André Juliard, with a preface by Marcel Boll. 140 pages, bibliography and index, 42 illustrations, 12mo. Paris, Gauthier-Villars et Cie., 1924. Price, in paper, 12 Francs.

This is an elementary exposition of the modern theories of atomic structure; the main text avoids the mathematical demonstrations, which are set forth in a supplement for the benefit of those who are able to utilize that phase. Probably no science has undergone such radical modification of its fundamental and long accepted theories as has chemistry in recent years. The discoveries in the field of radioactivity, the isolation of isotopes and the elucidation of the constitution of the atom, have given to modern textbooks a character wholly different from that which they had a generation ago. Yet, in the practical field, in the analytic laboratory and industrial establishment, these revolutionary developments remain essentially academic. The analyst who determines the

amount of mercury in the contents of a stomach or the amount of lead in an ore gives himself (or herself) no concern about the possible isotopes that may make up the aggregate, nor how many electrons may be in the atom or how they may be arranged. The time may come, however, when these abstruse features have an important bearing on the applications of chemistry, and it behooves the younger workers in the science, and especially teachers, to acquaint themselves with the general features of the newer views. Marcel Boll, in the introduction, presents in a concrete form some of the doctrines of the modern chemical philosophy. "The universe is, for unknown reasons, sown with volumes in which energy is greatly condensed, to which the term 'matter' is applied. This form of energy is resolved into two portions, differently electrified, the 'electron' which carries the negative charge and the 'nucleus' which is positive." "Atoms are systems electrically neutral. The electron has so far not been decomposed, but the nucleus seems to be an aggregation of electrons (nuclear electrons) with positive corpuscles (protons). The proton and electron are, therefore, the fundamental constituents, the proton itself is identified with the most simple nucleus, that of hydrogen."

These quotations will serve to show how far the old views of the indestructibility and indivisibility of the atom have been changed. The work in hand sets forth in simple language the modern views on the subject, the mathematical consideration being given, as noted in an appendix. For those who read French it is a very useful summary of the field. It presents the acceptable feature, rare in French books, a good index. This may be due to the fact that it is a translation. On the other hand, the "Table des Matières," so characteristic of French books, generally taking the place of an index, is missing.

HENRY LEFFMANN.

RADIOACTIVITY, AND THE LATEST DEVELOPMENTS IN THE STUDY OF THE CHEMICAL ELEMENTS. By Kasimir Fajans, University of Munich. Translated from the fourth German edition by T. S. Wheeler, B.Sc., and W. G. King. xvi–138 pages, 11 diagrams and 14 tables, 8vo. New York, E. P. Dutton and Company, no date. Price, $3.50 net.

The discovery of radioactivity by Becquerel in 1896 found a prompt application in physics, but chemistry was not influenced until later. To-day the fundamental phenomena and associated data are appearing in the textbooks of elementary chemistry, and have had the result of turning the attention of research workers to physical chemistry to such an extent that it seems possible that ere long the science which has been actively pursued for milleniums will be absorbed into physics. Professor Fajans published the first edition of this work in 1919, and the fact that it has gone to a fourth edition in four years is evidence of the manner in which it meets the demand for information on the most active phases of development in chemical philosophy. The translation into English is, therefore, a benefit to the American and British student, for, in spite of its merits, German is a difficult language to those who have not been taught it in infancy. Those who desire to get the latest trustworthy developments not only in radioactivity proper, but concerning isotopes and the structure of the atom, will find the book very opportune, but in justice it should be said that the essays by Aston and Thomson cover nearly the same field and quite

satisfactorily. The rapid and extensive development of new knowledge has necessitated the coining of new terms, among which are proton, electron, isotope and isobare. Fajans adds another word "pleiad" which he applies to a given group of isotopes. The term is derived, of course, from the well-known star group, a characteristic feature of the constellation Taurus. It is quite appropriate and suggestive, but it may be well for teachers who employ it to inform students that the celestial eponym is not necessarily a group of related stars, but merely such as happen to be in about the same line of sight. The members of the group may be at great distances from each other and wholly independent.

The work presents at some length the Rutherford-Bohr theory of the structure of the atom, according to which each atom consists of a positively charged nucleus surrounded by negative electrons. As these latter, notwithstanding their attraction for the positive nucleus, remain at a distance from it, it must be assumed that they are in motion in orbits, so that each atom becomes an epitome of the solar system, a microcosm in fact. This concept of atomic structure seems to differ somewhat from that which Marcel Boll sets forth in his introduction to the recently issued French translation of Bavink's work on "Atomistics." Boll states that the nucleus seems to be made up of electrons (nuclear electrons) and positive corpuscles identical in nature with what is termed the "proton" and is the atom of hydrogen. In the Rutherford-Bohr model, the electrons describe circular or elliptical orbits. An inference from such a view seems to the reviewer to be that the Newtonian law of inertia, which manifests itself as centrifugal force, is operative within the atomic mass. Fajans states that a simple arrangement is possible only in atoms having a moderate number of electrons; in the more complicated ones, the electrons are not in simple spatial relations but in different associations. In the light of this theory, Fajans gives an interesting and valuable suggestion as to the nature of the hydrogen-ion, to which so much attention has been directed in physiologic chemistry recently. The hydrogen atom consists of "one positive elementary quantum around which rotates one negative electron. If the atom loses this electron the remainder is the simple positively charged hydrogen-ion, and the smallest known positive charge. The mass of the hydrogen atoms seems to be inseparable from this charge, for the hydrogen-ion is the lightest positively charged particle that has yet been observed. This 'positive electron' has a mass 1836 times greater than the negative electron." The expression "positive electron," which is in quotation marks in the book and is doubtless directly from the original, will puzzle a little the beginners in the study of this subject, as it is generally understood that the electron is the negative charge. The phrase "lightest positively charged particle" is also somewhat ambiguous, as "lightest" may in English qualify either "positively charged" or "particle." Presumably the latter is the case.

The book contains a large amount of matter relating to the data and principles that have been developed from the researches into the remarkable and interesting phenomena of radioactivity, and will be useful in its English dress to a large group of students and teachers. It is well printed, on good paper, with clear type, and the translation is excellently done.

HENRY LEFFMANN.

ARSENIC, CALCIUM ARSENATE AND THE BOLL-WEEVIL. Articles and addresses by Howard W. Armbruster. Pamphlet, 42 pages, 8vo. New York, Barr-Erhardt Press, Incorporated. Price, $.50.

Some years ago an industrious and prolific mother crossed the Rio Grande and made a home in Texas, raising a family, which has spread over a large area of the South. Scientifically termed *Anthonomus grandis*, it is more commonly known as the "boll-weevil." It specializes in consuming the cotton boll before ripe, and has devastated a large area of the "cotton belt." Incidentally, it is probably responsible for much of the emigration of the southern negro to the northern states, as the labor in the cotton field has become unavailable to him, and the economic conditions of the South do not offer opportunities for other phases of industrial occupations. Mr. Armbruster's essay consists of reprints of papers and addresses by him dealing with the supply of arsenic, tariff and other connected problems. The connection with the boll-weevil problem is that calcium arsenate has so far seemed to be the only substance that will effectually control the insect. A shortage of arsenic is threatening, and the question of securing a more abundant supply needs careful consideration. The manufacture of insecticides is one of the most important uses of arsenic, but a notable amount is used in glassmaking. About 16,000 tons are used annually in the United States. The bulk of the supply has come as a by-product of smelting ores of precious metals. Formerly, the arsenical fumes were allowed to escape, but litigation brought about restriction and the Cottrell process facilitated recovery, so that the supply has been fairly abundant.

The pamphlet gives a wide and interesting survey of the arsenic problem in connection with the boll-weevil campaign, and shows that the development of an efficient industry and judicious control of tariff and other incidental business methods must be brought about if the financial loss is not to be serious. The insect so far has not imperilled the food supply, but, if cotton ceases to be cultivated in large amount, it may turn its attention to other plants. The war on insect pests is widespread and no armistice is in sight. Mr. Armbruster deserves credit for his devotion to the problem and his pamphlet is timely and instructive.

HENRY LEFFMANN.

QUANTITATIVE ANALYSIS. Containing theory, laboratory directions, problems, explanations of calculations and special topics in analytical chemistry. By Stephen Popoff, Ph.D., University of Iowa. xiii–342 pages, 28 illustrations, 12mo. Philadelphia, P. Blakiston's Son and Company, 1924. Price, $2.25 net.

It is interesting to compare this book with Fresenius' "Quantitative Analysis" that was so familiar in the analytic laboratory half a century ago—the analyst's "Bible" it was sometimes called. Fresenius gave scarcely any attention to the theory of the processes of analysis, indeed, in those days the theories were scanty and of but little application. It is, indeed, said of the great Wiesbaden chemist that he had deliberately relinquished interest in the theoretical phases of the science on account of the general acceptance of the unitary system, just as Cavendish is said to have abandoned his work because of the downfall of the phlogiston theory. Doctor Popoff remarks the practice of analytic chemistry does not require a knowledge of the principles of the

science. A worker, who is neat, careful and honest, may be given a routine, and will get results that are as reliable for practical purposes as the results of a recipient of a Nobel prize. The progress of any science, however, depends upon the coördinate development of its pure and applied phases. Furthermore, specific distinctions in a science, as doctorate and baccalaureate degrees, and even the minor ones, such as A.C., C.E., and Ph.G., should signify considerable acquaintance with the theories of the science appertaining.

The book in hand goes deeply into the modern theories of chemistry, interpreting the reactions on the most advanced basis. Every effort has been made by the author to make clear the recondite and abstruse principles, but some students may find difficulty in comprehending the theories no matter how carefully set forth, and follow the example of Fresenius, paying attention to the routine methods and giving no thought to the why and wherefore thereof. The book is limited to inorganic analysis, in fact to that field which in earlier years was termed "mineral" analysis, for only the more familiar elements are included. This is entirely proper, for it is intended primarily for training students, and, therefore, gives elaborate directions for the care and use of apparatus, for methods of manipulation and advice as to the duties and responsibilities of the analyst. The book is carefully written, freely and appropriately illustrated, well printed, and has abundant references to the literature. In a special, extensive and comprehensive bibliography on Electrometric Determinations, the year and the volume are given, which is the proper method. Tables and diagrams add to the usefulness of the work, which is a valuable contribution to the literature of analytic chemistry. One questionable procedure is the use of the word "normal" in a sense very different from that in specific use in chemistry. Doctor Popoff uses it, as opposed to "abnormal," in contrasting the properties of a solution of sugar in water or hydrochloric acid in toluene, which give normal data on osmotic pressure, freezing point, etc., with solutions of ionizable substances that are not normal in those respects. It is, however, unwise to use a word in two technical senses in the same science.

HENRY LEFFMANN.

COLLOID SYMPOSIUM MONOGRAPH. Papers and discussions presented at the First National Symposium on Colloid Chemistry at the University of Wisconsin, June, 1923. Edited by J. Howard Mathews, University of Wisconsin. 8vo, 410 pages. Published by the University, 1923. Price, $2.50.

No department of applied science seems to have grown so fast and so broadly as the chemistry and physics of colloids. It has so many enthusiastic investigators and has so many fascinating features that it is actively cultivated in all countries in which science is in vogue. The immediate incentive to the symposium reported in the volumes in hand was the presence of Professor Svedberg, of Upsala, who was at the University of Wisconsin, and it was deemed appropriate to hold such a conference while he was in the country. The papers are numerous and instructive, covering a wide range of theory and practice. It may be that some of the votaries in this field have become too enthusiastic. Some time ago the reviewer read Wolfgang Ostwald's "Colloidchemie" in the original (no easy task) and felt at the finish as if the author might be willing to believe that the primordial chaos was a colloid and that

the fiat of creation was a flocculation thereof. Truly, however, the data of colloid chemistry are far-reaching and of the greatest practical and theoretical importance. Much of the present knowledge is set forth in this volume, the essays covering a wide field. Several of the papers read at the symposium do not appear, having been assigned for publication elsewhere. It is not possible to set forth here any abstracts of the communications. They are naturally highly specialized, but it can be said that all are worthy contributions and in its entirety the volume is a valuable contribution to the literature of the subject. One point in criticism must be noted. In several places "benzene" is called "benzol." This is quite unjustifiable. The word "benzol" has no appropriateness to the molecule C_6H_6. It is merely carrying over into modern nomenclature a word from the German, and violating the basic principle of nomenclature in organic chemistry, according to which "ol" indicates alcoholic structure. The use of the word "benzol" in modern chemical literature is not due to ignorance or to any definite system of terms, but merely to a petty obstinacy which tends to ignore the rules applicable to other fields than those in which the offender in this respect is interested. The editor, whose work is otherwise well done, should have corrected the erroneous term, especially as he has allowed the correct word to appear in at least one place. A systematized phraseology is necessary to any book and strict adherence to its rules obligatory. The volume is well-printed on good paper. Arrangements have been made for a similar symposium during the current year at Evanston, under the auspices of the Northwestern University.

HENRY LEFFMANN.

CHEMISCH-TECHNISCHE VORSCHRIFTEN. By Dr. Otto Lange, Technical High School, Munich. Third edition, enlarged and thoroughly revised. Vol. 3, Resins, Oils and Fats. xix–796 pages, large 8vo. Leipzig, Otto Spamer, 1923. Price, 45 gold marks, net.

The first two volumes of this monumental work have already been noticed and little remains to be said of the present one, except that it maintains the high standard already reached. The subject-matter covers a considerable portion of the most important departments of applied organic chemistry. Oils, fats and resins are extremely numerous and find an almost infinite variety of applications, since, unlike many other raw materials that come within the scope of industrial chemistry, they enter largely into human diet. It is now more than a century since Chevreul elucidated the fundamental nature of the fats and oils, and along the same lines of investigation, the resins have been studied.

The book opens with a chapter on rubber. The term "caoutchouc" is a modification of a word used by the natives of the Amazon region. It is used in somewhat modified spelling in most languages, but English has settled on "rubber," and this will probably remain. Two matters of special interest attach to the chemistry of rubber, the enormous growth of the plantation industry and the efforts at securing a synthetic product. Gutta-percha (a Sumatran term) is of the same general nature as rubber, that is, a latex. Many plants bear abundant latex in their stems and leaves, and much remains to be done in investigating the nature of these, but it is not likely that commercial uses will be found in most cases. The floras of temperate climates yield numerous

latex-bearing plants, among which the milkweeds, dogbanes, sumacs and spurges are quite familiar. W. H. Perkins, Jr., is quoted as enumerating three lines along which synthetic rubber may possibly be prepared. (1) Production of isoprene, which can be obtained from amyl alcohol. (2) Transformation of butyl alcohol. (3) Transformation of aldehyde into butadein. These methods will become commercially successful only when the starting points as designated are capable of being produced at low cost. Yet we must not be too pessimistic, but bear in mind what has been accomplished with alizarin, indigo and vanillin.

The general make-up of this book is similar to the earlier volumes. The arrangements are convenient, the references to patent and journal literature excellent and comprehensive, the table of contents elaborate and the index most creditable. The possessor of this series has a most satisfactory compendium of the present state of industrial chemistry, upon which implicit reliance may be placed.

HENRY LEFFMANN.

NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS. Report No. 174, The Small Angular Oscillations of Airplanes in Steady Flight, by F. H. Norton, Langley Memorial Aeronautical Laboratory. 8 pages, illustrations, quarto. Washington, Government Printing Office, 1923.

This investigation was carried out by the Committee at the request of the Army Air Service to provide data concerning the small angular oscillations of several types of airplanes in steady flight under various atmospheric conditions. The data are of use in the design of bomb sights and other aircraft instruments. The method used consisted in flying the airplane steadily in one direction for at least one minute, while recording the angle of the airplane with the sun by means of a kymograph. The results show that the oscillations differ but little for airplanes of various types, but that the condition of the atmosphere is an important factor. The average angular excursion from the mean in smooth air is 0.8° in pitch, 1.4° in roll, and 0.9° in yaw, without special instruments to aid the pilot in holding steady conditions. In bumpy air the values given above are increased about 50 per cent.

Report No. 178, Relative Efficiency of Direct and Geared Drive Propellers. By Walter S. Diehl. Nine pages, illustrations, quarto. Washington, Government Printing Office, 1923.

This report is an extension of Technical Report No. 168 and has been prepared for the Committee to show the relative values of various direct and geared drives. It has been assumed that the speed V and the crankshaft revolutions are held constant at each value of $\left(\frac{V}{ND}\right)_2$ corresponding to the maximum efficiency for a two-bladed, direct-drive propeller, so that the corresponding $\left(\frac{V}{ND}\right)$ and maximum efficiency for any other propeller arrangement depends only on N and D, which are easily calculated. The net efficiencies are obtained by allowing 98 per cent. for the gears and 95 per cent. for the efficiency of a four-bladed propeller relative to a two-bladed propeller.

The net efficiencies so found are given in terms of the efficiency for the two-bladed, direct-drive case, and plotted against $\left(\frac{V}{ND}\right)_2$ so that having given

the $\left(\frac{V}{ND}\right)$ corresponding to maximum efficiency for a two-bladed, direct-drive propeller, the relative gain or loss due to any ordinary arrangement may be readily estimated. The conclusion is reached that when $\left(\frac{V}{ND}\right)_2$ is greater than 0.70, gearing is not advisable.

Report No. 179, The Effect of Electrode Temperature on the Sparking Voltage of Short Spark Gaps. By F. B. Silsbee, Bureau of Standards. Ten pages, illustrations, quarto. Washington, Government Printing Office, 1923.

This report presents the results of an investigation carried on at the Bureau of Standards at the request of the Committee to determine what effect the temperature of spark-plug electrodes might have on the voltage at which a spark occurred. A spark gap was set up so that one electrode could be heated to temperatures up to 700° C., while the other electrode and the air in the gap were maintained at room temperature. The sparking voltages were measured both with direct voltage and with voltage impulse from an ignition coil. It was found that the sparking voltage of the gap decreased materially with increase in temperature. This change was more marked when the hot electrode was of negative polarity. The phenomena observed can be explained by the ionic theory of gaseous conduction, and serve to account for certain hitherto unexplained actions in the operation of internal combustion engines.

These results indicate that the ignition spark will pass more readily when the spark-plug design is such as to make the electrodes run hot. This possible gain is, however, very closely limited by the danger of producing pre-ignition. These experiments also show that sparking is somewhat easier when the hot electrode (which is almost always the central electrode) is negative than when the polarity is reversed.

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Modern Electro-plating. A guide-book for platers, works chemists, and engineers, by W. E. Hughes. Oxford Technical Publications. 160 pages, plates, 8vo. London, Henry Frowde and Hodder and Stoughton, 1923.

Clouds and Smokes. The properties of disperse systems in gases and their practical applications, by William E. Gibbs. Foreword by Sir Oliver Lodge. 240 pages, illustrations, plates, 8vo. Philadelphia, P. Blakiston's Son and Company, 1924. Price, $3.

Pulverized Fuel. A Practical Handbook, by W. Francis Goodrich. 215 pages, illustrations, plates, 12mo. London, Charles Griffin and Company, Ltd.; Philadelphia, J. B. Lippincott Company, 1924.

American Typefounders Company, Specimen Book and Catalogue. 1148 pages, 8vo. Jersey City, Company, 1923.

U. S. Department of Agriculture, Weather Bureau. Daily river stages at river gage stations on the principal rivers of the United States. Vol. xx for

the year 1922, by H. C. Frankenfield, Meteorologist. 268 pages, quarto. Washington, Government Printing Office, 1923.

U. S. Department of the Interior, Bureau of Mines. Analytical methods for certain metals, including cerium, thorium, molybdenum, tungsten, radium, uranium, vanadium, titanium and zirconium, by R. B. Moore and S. C. Lind, J. W. Marden, J. P. Bonardi, C. W. Davis and J. E. Conley. 325 pages, plates, tables, 8vo. Washington, Government Printing Office, 1923.

Monograph on Corrosion Tests and Materials of Construction for Chemical Engineering Apparatus, by W. S. Calcott, J. C. Whetzel and H. F. Whittaker. Reprinted from vol. 15, part 1, of the *Transactions of the American Institute of Chemical Engineers*. 182 pages, illustrations, 8vo. New York, American Institute of Chemical Engineers, 1923. Price, $3.

Quantitative Analysis Containing Theory, Laboratory Directions, Problems, Explanations of Calculations and Special Topics in Analytical Chemistry, by Stephen Popoff. 342 pages, illustrations, 12mo. Philadelphia, P. Blakiston's Son and Company, 1924. Price, $2.25.

Treatise on General and Industrial Organic Chemistry, by Ettore Molinari. Second English edition. Translated from the third enlarged and revised Italian edition by Thomas H. Pope. Part II, 897 pages, illustrations, 8vo. Philadelphia, P. Blakiston's Son and Company, 1923. Price, $8.

Allen's Commercial Organic Analysis. Vol. 1, Introduction, alcohols, yeast, malt and malt liquors, wines and potable spirits, neutral alcohol derivatives, sugars, starch and its isomerides, paper and pulp testing, aliphatic acids. Fifth edition, revised and in part rewritten. Editors, Samuel S. Sadtler, Elbert C. Lathrop, C. Ainsworth Mitchell. 769 pages, illustrations, 8vo. Philadelphia, P. Blakiston's Son and Company, 1923. Price, $7.50.

National Advisory Committee for Aeronautics. Technical Notes, No. 174, Airplanes in Horizontal Curvilinear Flight, by Heinrich Kann. 18 pages, illustrations, quarto. No. 175, Tests on a Model of the D Airplane T39 of the Deutsche Flugzeng Werke, by Wilhelm Molthan. 19 pages, illustrations, quarto. No. 176, Curvilinear Flight of Airplanes, by E. Salkowski. 21 pages, illustrations, quarto. No. 177, Note on the Relative Effect of the Dihedral and the Sweep Back of Airplane Wings, by Max M. Munk. 4 pages, quarto. Washington, Committee, 1924.

The Status of Research in Ore Dressing, by Ernest A. Hersam. Report to the Milling Committee of the American Institute of Mining and Metallurgical Engineers and the Bureau of Mines. 48 pages, quarto. New York, 1924.

The Velocity of Propagation of Electromagnetic Waves Along Conducting Wires. M. MERCIER. (*Ann. de Phys.*, Vol. 20, 1923.) —For the method an accuracy of one part in 10,000 is claimed yet the velocity as measured is 1/2000 less than the velocity of light waves in air. To account for this discrepancy recourse is had to a possible change in the electrical resistance of the superficial layers of the wires. The nearer together the wires are the smaller does the velocity of the electrical waves appear to be. Moreover the thicker the wires the greater does the velocity seem to be. G. F. S.

CURRENT TOPICS.

The Photoluminescence of Flames. E. L. NICHOLS and H. L. HOWES. (*Phys. Rev.*, Nov., 1923.)—"The brightness of the flame plus the light transmitted by the flame from a nitrogen-filled tungsten lamp placed behind it, should normally be slightly less than the sum of the two intensities taken separately, whereas instead of showing a loss by absorption the combined brightness was persistently greater." After failure of other explanations that suggested themselves the authors were driven to conclude "that the flame was actually somewhat brighter when exposed to the light of the neighboring source." They proceeded to test this hypothesis and found that the facts agreed with it. An air-hydrogen blast lamp was used with Li, Na, Sr and Ca salts. Light from a tungsten lamp, a carbon arc, a mercury arc and an iron spark fell in turn upon the flames. "The iron spark was most effective, the increases being over 10 per cent. for Ca and Sr. Not only the ultra-violet beyond .3μ is effective but also light above .43μ." On the other hand, red or yellow light of such wave-length that it was absorbed by the flame reduced the emission of the flame by a few per cent. No matter whether the effect of the incident light was to enhance or to reduce the emission, it seemed to produce the same effect upon all the lines derived from the flame. When a calcium band was observed while its parent flame was illuminated both by light through ruby glass and by light from an iron spark its brightness was greater than that of the flame alone but less than that of the same flame exposed to the spark light alone. The flame of the Bunsen burner showed scarcely any change due to illumination, and the different parts of the hydrogen flame were sensitive in different degrees. G. F. S.

The Application of Optical Methods to the Examination of Works of Art. E. BAYLE and HENRI GEORGE. (*Comptes Rendus*, Jan. 2, 1924.)—For the production of light of a desired composition a projector of four beams is used. Three of these have respectively red, blue and green screens. The fourth has white light. By a combination of these beams a great number of shades of colored light may be had. By the use of the proper shade peculiarities of the painting may be photographed. "It is thus that we have been able to get very clear evidence in the case of an Impressionist master of the form of the strokes imposed upon him by a physical disability." By the employment of an ultra-violet light, which makes varnish fluoresce, the quantity of this material, as well as particular treatment, is revealed. By this means a picture signed "Goya a. 1799" was discovered to have another inscription to the effect that it was a copy. Spectroscopic methods are used in the study of the colors on the painting. Mercury is thus determined and one-fourth mg. of paint suffices

to bring out lead lines in the spark. The microscope is valuable in the examination of mixed pigments. The spectrophotometric composition of the light diffused from Prussian blue, cobalt blue, madder, etc., is known. These methods are supplemented by the information furnished by X-rays. G. F. S.

AT THE meeting of the British Association for the Advancement of Science in 1923 Sections A, B, and G united in a joint discussion of cohesion and molecular forces. Very appropriately Sir William Bragg took up the matter of crystal structure. The old view that atoms and molecules exert in all directions forces that vary with some power of the distance has had success in explaining certain phenomena but has largely failed with solids. "A crystal conforms so exactly to rules respecting its angular dimensions that it seems impossible to imagine its form to be merely the result of an average of tendencies. The forces of adjustment cannot, therefore, be thought of as a force between two points each representing one of the molecules. On the contrary, it is nearer the truth to think that the adjustment is made so as to bring together certain points on one molecule and certain points on the other. In considering, therefore, the binding of the individual molecules of a solid, the analogy of the attraction of two charged spheres is imperfect, and should be replaced by that of two members of a girder structure adjusted until the rivets can be dropped into the holes brought into true alignment. . . . One well-known fact in crystal growth is that the faces have different rates of growth, indicating that there may be great differences in the ease with which molecules slip into their places."

After the subject of cohesion had been discussed by several speakers it was clear that no more than a beginning has been made. "There was point in the somewhat facetious remark of Sir Oliver Lodge that it was an extraordinary fact that, after all these years, three important sections of the British Association should be gathered together to discuss why, when one end of a stick is raised from the table, the rest of it also comes up." (*Nature*, Nov. 24, 1923.)

G. F. S.

The Production of an Electromotive Force by Illumination. P. H. GEIGER. (*Phys. Rev.*, Nov., 1923.)—When a piece of argentite, Ag_2S, is put in circuit with a galvanometer and one contact surface of the mineral is illuminated, an electromotive force is produced. Illumination of the other face reverses the direction of the current. Specimens from different localities all showed this property. The electromotive force increases with the intensity of illumination until a maximum of .013 volt is reached. The effect depends on the wave-length of the incident light, the maximum result being attained for a wave-length of 1μ. Six other minerals gave the effect to some extent, while others, in which illumination produces a change of electrical resistance, failed to show any electromotive force.

G. F. S.

The Spectrum of Scattered X-rays. A. H. COMPTON. (*Phys. Rev.*, Nov., 1923.)—Upon the basis of the quantum theory this investigator has recently proposed a conception of the scattering of X-rays that leads to the conclusion that the increase in wave-length due to the process is proportional to unity minus the cosine of the angle at which scattering takes place. To test this relation a special tube with a molybdenum target was designed to give scattered rays of unusual intensity. Graphite was the substance that caused the scattering. In all cases the beam of scattered rays was found to consist of two parts, the original rays and rays of slightly longer wave-length. As the angle of scattering increased so did also the interval between the wave-lengths of these two components and this interval was in accord with the value predicted by the formula of the author. Neither of the two components was made up of a single wave-length, but the range of wave-lengths was less for the unchanged component than for the other. The change of wave-length due to scattering is always an increase. G. F. S.

Health of Workers with Radium.—R. C. WILLIAMS (*Public Health Rep.*, 1923, 37, 3007–3028) kept under observation for a period of eighteen months twelve persons who were constantly engaged in the handling of radium preparations. At least two persons presented symptoms showing evidence of the effect of radium radiation upon the skin of the fingers and hands. Dental films worn by these persons demonstrated their exposure to the radiation. Their blood-pressure decreased, and the total number of both red and white blood-cells tended to decrease. Changes also occurred in the differential blood-count, or the ratio of the different types of white cells (leucocytes) to each other. Recommendations for the protection of the health of workers with radium include frequent physical examinations and determinations of blood-pressure and blood-count, protection from exposure to the radiation to the greatest possible extent, a midweek holiday, and a vacation of two weeks semi-annually. J. S. H.

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